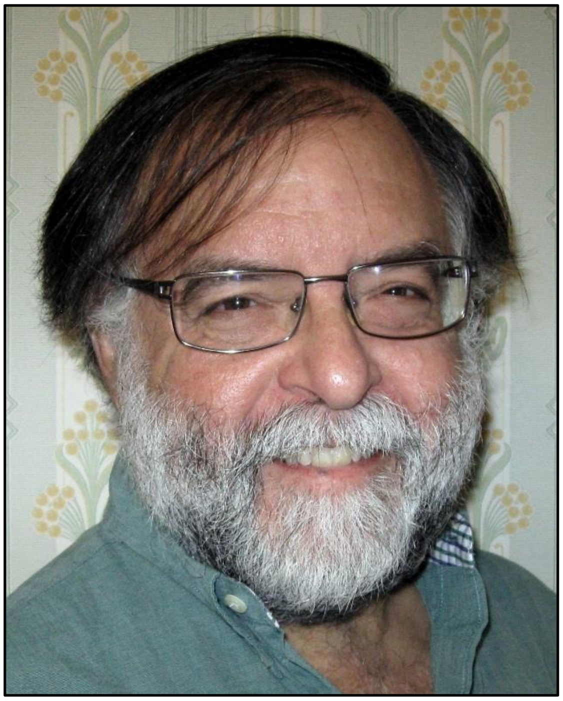

STANLEY E. WHITCOMB
(b. 1951)

INTERVIEWED BY
HEIDI ASPATURIAN

April – June 2017

ARCHIVES
CALIFORNIA INSTITUTE OF TECHNOLOGY
Pasadena, California

Subject area

Physics, LIGO

Abstract

Interview in five sessions, April–June 2017, with Stanley Whitcomb, chief scientist with LIGO (Laser Interferometer Gravitational-Wave Observatory) and one of the longest-serving principals on the project, having been at various times deputy director, R&D director, detector group leader, and acting director. These interviews, a follow-up to a 1997 interview [http://resolver.caltech.edu/CaltechOH:OH_Whitcomb_S], were conducted about 18 months after LIGO made its landmark detection in September 2015 of gravitational waves from colliding black holes, confirming a key prediction of Einstein's general theory of relativity. The first session deals with Whitcomb's account of the discovery and its aftermath, with sessions 2 through 5 focusing on his involvement with LIGO from the 1990s to the present day. He describes LIGO's evolution from a modest scientific undertaking to a Caltech-MIT-NSF mega-collaboration with hundreds of personnel at multiple institutions, and recalls

the organizational, administrative, and technical changes that accompanied this transition. There is extensive discussion of the roles played by B. Barish, R. Drever, J. Marx, D. Reitze, K. Thorne, R. Vogt, and R. Weiss, and numerous others who made essential contributions to LIGO's success. He recalls the doubts and controversies that swirled around the project, especially in its earlier phases, and offers his thoughts on the factors that kept it viable and moving forward despite these challenges. He talks about his multifaceted administrative responsibilities, including his work with LIGO's Livingston and Hanford observatories, particularly the latter, and his tenure as head of the detector group charged with developing and installing LIGO's unprecedented optics and other innovative technologies. He charts LIGO's progress from Initial to Advanced LIGO, the establishment of the LIGO Scientific Collaboration, and international outreach efforts, including overtures to Australia and the creation of LIGO–India. His personal reflections on LIGO's scientific and historic significance also form part of this oral history.

Administrative information

Access

The interview is unrestricted.

Copyright

Preferred citation

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CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES

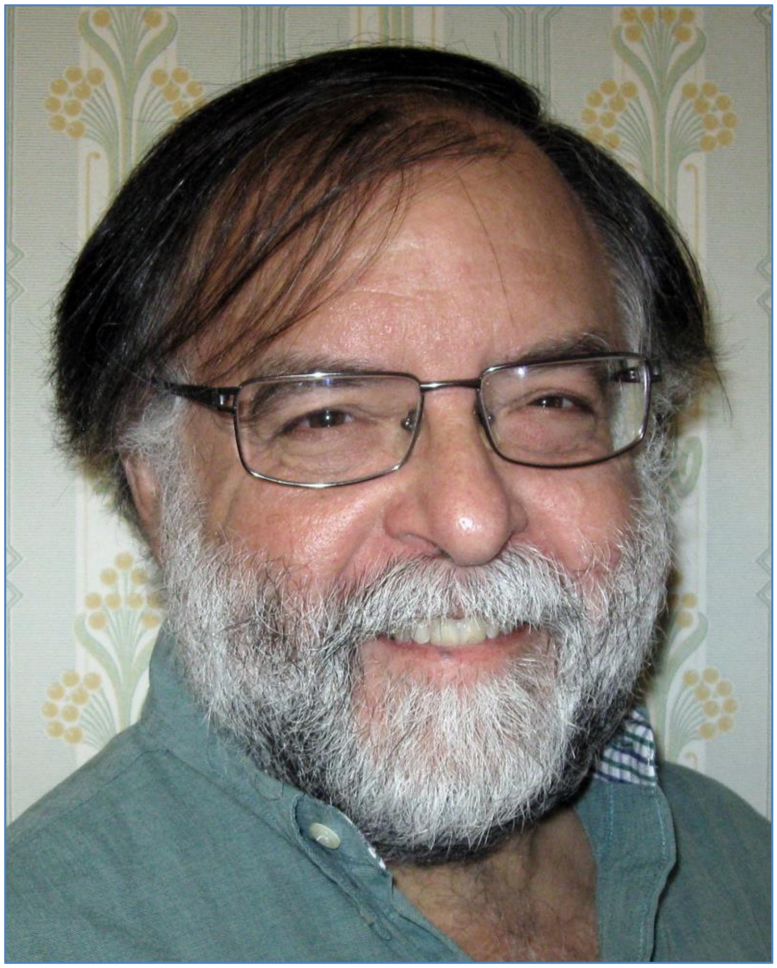
ORAL HISTORY PROJECT

INTERVIEW WITH STANLEY WHITCOMB

BY HEIDI ASPATURIAN

PASADENA, CALIFORNIA

http://resolver.caltech.edu/CaltechOH:OH_Whitcomb_S_Ligo_2017



Stanley Whitcomb

FORWARD

Embedded in the sidewalk leading to the main New York City Public Library, there are a series of brass plaques with quotes from various sources, all appropriate to the role of libraries in society. I came across the following one in the summer of 2016:

> *why some people be mad at me sometimes*
> by Lucille Clifton
>
> they ask me to remember
> but they want me to remember
> their memories
> and i keep on remembering
> mine

Coming shortly after the announcement of the first LIGO detection, at a time when people were starting to ask and write about the history of LIGO, this poem resonated with me.

This oral history has been almost two years in the making. I was first approached by Heidi Aspaturian of the Caltech Archives in January 2017 asking if I would be willing to sit for a series of interviews about LIGO. We soon realized that the earlier interviews I had completed in 1997 had never been officially published, stalled on my side waiting for my final edits and approval. We agreed that we would first finish and publish the 1997 transcript [http://resolver.caltech.edu/CaltechOH:OH_Whitcomb_S] and then we would continue with a second round of interviews. Those interviews, five additional sessions, took place in spring 2017. They were transcribed later that year, and we had a back and forth exchange of edits to produce this document.

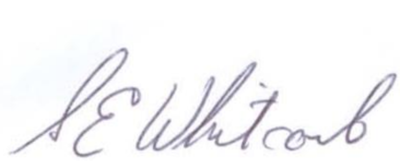
Stepping back and looking at the set of interviews as a whole, I would characterize it as a small, inside glimpse into LIGO rather than a "grand history" of the project. Those who read this expecting to learn how the project was conceived, how the big decisions were made about who would lead it, how it would be structured and managed, and so on, are likely to be disappointed. Caltech and MIT presidents and NSF directors appear only incidentally, not because they didn't play a major role in the history of LIGO, but because my interactions with them were limited. The fact is, in most cases I had as limited a view of how the institutional leaders made the decisions that steered the project as they had about how the decisions concerning the detector design were made. For those who want it, that grand history of LIGO in its earlier stages is recounted in some of the other oral histories in the Caltech Archives.

The battle to make LIGO a success was fought on many different levels: at the level of the grand history, of course, but also on a working level, where the daunting technical challenges of building an instrument that many outsiders believed could not be built were faced and vanquished, and on a personal level, where a team of scientists and engineers had to soldier on for years without the usual scientific rewards. I would like to think that this look into those other aspects is a tribute to those who labored at the working level, though to be honest, I feel it falls a bit short of my goal in that respect. Next time, perhaps. . . .

I expect that many of the readers of this oral history will be my friends and colleagues from LIGO, and I anticipate a couple of reactions from them. Many will ask how I could leave out Person X or not talk about the ABC incident. To them I would point out the impossibility of condensing more than 35 years of labor on LIGO into approximately five hours of interviews without leaving out numerous important people and events. Many organizations and people who played a significant role in my working life (and whose contributions I hold dear) do not even appear here, let alone get the recognition they deserve. The precise mixture of events and people that appear in this oral history is much more a matter of chance than we might at first think—driven by the confluence of the questions Heidi asked in a particular session, by what I was thinking about in the days and weeks before each session, etc. It is not an experiment that we will ever do, but I suspect that if Heidi and I were to sit down to repeat this exercise, the final product of those interviews might have less than a 50 percent overlap with this one.

The other reaction that I expect is that some people may disagree with my version of events as recounted here. To them, I recommend reading again the Lucille Clifton poem at the beginning of this Forward. The more I learn about how "elastic" our memories can be, the less I am inclined to take anyone's account of the past as completely correct, and I would advise readers not to accept my version of events without question either. There are significant places where my memories or impressions may differ from those of others. Truth may lie in one version or the other, somewhere in between, or even in some completely different place. What I can say is that this is the version I remember.

Finally, I'd like to thank and compliment Heidi Aspaturian for her role in making this happen. In preparation for these interviews, I went back and reread some of the earlier LIGO oral histories from the Caltech Archives, mostly from the '90s. One of the things that struck me from that rereading was how much influence the interviewer has on the outcome, both through the questions that are asked and through what gets followed up and what doesn't. I felt that Heidi's questions were insightful and well researched, and her instinct about when to push for just a bit more on a topic was generally spot-on. I confess that I started the whole business with some trepidation, but by the end I was looking forward to the next session. Thank you very much, Heidi!



January 2019

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CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES
ORAL HISTORY PROJECT

Interview with Stanley Whitcomb **by Heidi Aspaturian**

Pasadena, California

Session 1	**April 24, 2017**
Session 2	**May 8, 2017**
Session 3	**May 22, 2017**
Session 4	**June 5, 2017**
Session 5	**June 19, 2017**

SESSION 1
April 24, 2017

ASPATURIAN: This is April 24, 2017, and this is oral history interview session number one with Dr. Stanley Whitcomb. We are going to talk today about the LIGO discovery and its aftermath. I'd like to start by quoting from a very good interview you and Barry Barish [Linde Professor of Physics, emeritus; 2017 Nobel laureate in physics] did with Doug Smith [formerly of the Caltech public relations office] back in May of 2015. At the very end of it, Doug said, "Well, when do you think we're going to see something?" And Barry said, "I've always had the fond wish we'd do it by 2016." And then he speculated that it might be within five years, maybe no more than ten years. And lo and behold, in less than a year, this discovery had come about. So let's talk about where you were and how you found out, and then we'll take it from there.

WHITCOMB: OK. I probably learned about it about as early as most people on the West Coast did. I oftentimes don't sleep well at night, and this was a time when I wasn't sleeping well, so it was, if I recall correctly, a Monday morning, and I had gotten up around 4 a.m., fretting about something. I should perhaps tell you that my last day before retirement was scheduled for Tuesday. I was going to sign the paper and walk away with my final paycheck on Tuesday afternoon. And then I was planning to go away on vacation on Wednesday.

So I was juggling lots of little things in my mind, which is why I was awake at that hour. I had an email to send that was bugging me, so I got up and went over to my laptop, sat down, composed that email, and sent it off into the ether—I don't even remember exactly to whom it was sent. Then I thought, "Well, as long as I'm awake and I've got my computer fired up, I'll just quickly check my email so that I can see what else is going on." And I came across the email announcing that there was this interesting event seen by the LIGO detectors, and so I clicked through to see what the event looked like.

ASPATURIAN: Where had that email come from?

WHITCOMB: It was on one of the many email lists that I was on, but I don't remember exactly which one. I don't think I actually saw Marco Drago's original email, but it was one that was following up on that.

ASPATURIAN: He was the scientist in Germany?

WHITCOMB: Yes. The first person to notice this event.

ASPATURIAN: I've got a time here: 11 a.m. on September 14, 2015.

WHITCOMB: That may have been 11 a.m. their time, which was about 3 a.m. here, I think. So I was awake about 4 a.m., and I saw that email. I went and clicked through and then after I'd looked through the various plots and such, I went back to bed and slept for half an hour or forty-five minutes, something like that. Then my wife, Laurie, and I got up and walked our dog, as we did most mornings at that time. And as we were walking along, I told her that this retirement I was expecting to have, where I wasn't going to be doing anything much, probably wasn't going to work out quite the way I thought.

ASPATURIAN: Why did you say that? What had you picked up from looking at this event announcement?

WHITCOMB: Well, in my retirement I had agreed to do a small number of things for LIGO, one of which was to chair something called the LIGO detection committee. It's actually the LIGO–Virgo detection committee, and I was the LIGO co-chair. The role of that committee was to carefully evaluate any first detection that was made—to try to step back from the people who are doing the actual work and take a bigger-picture look at the whole case for a detection. We would look at what could possibly go wrong and at what pieces people who are working on this on a day-to day-basis might possibly miss. Are there any additional tests that should be done; is there any additional analysis; is the case really strong enough? All those kinds of questions.

ASPATURIAN: Was this something that had been planned in anticipation of an eventual discovery?

WHITCOMB: It had been planned for a long time. It goes back even to our first 2005 run with Initial LIGO, the S5 run. Even before that, many of us felt that the field had been badly burned by the experience of [Joseph] Weber [an early gravitational-wave scientist, whose claim to have detected waves in the 1960s and '70s could never be verified *–Ed.*]. Somewhat after that, [Guido] Pizzella [a physicist who led one of the Italian bar-detector efforts] and his team made some half-claims of possibly seeing something, again not confirmed. We wanted to avoid that pitfall, so we put in place something to check ourselves—"belt and suspenders." The idea was that this detection committee would be a skeptical group of "experts" who would be looking for any holes in the argument that a proposed candidate was a real gravitational-wave event. Rai [Rainier] Weiss [2017 Nobel laureate in Physics] had first established this—it was in the charter for the LIGO Scientific Collaboration [LSC], written when he was its first spokesperson, so it goes back to that timeframe. When we agreed to merge our data with that of Virgo, it became a joint LIGO–Virgo committee.

ASPATURIAN: Virgo being a detector in Italy?

WHITCOMB: Yes.

ASPATURIAN: And what does that acronym stand for?

WHITCOMB: It isn't an acronym; it doesn't stand for anything. [Laughter] The idea initially was that the Virgo detector would be able to see gravitational-wave sources out to the Virgo cluster of galaxies—I think that's where the name came from. But they're very clear that it's not an acronym. It's actually written with a capital V and lower case remaining letters, as a name, rather than an acronym. So the LIGO detection committee became a joint detection committee. In fact it has been exercised in the past—you may have heard of something called the Big Dog event, in the Initial LIGO run?

ASPATURIAN: I don't think so.

WHITCOMB: We actually had a system for making blind injections [injected signals that only a few members of the collaboration knew about], and we exercised it during the Initial LIGO run; and colloquially it became known as the "Big Dog," because the initial pointing for it was from the Canis Major constellation. And so the nickname Big Dog came along. As it happened, I was also chair of the detection committee during that time, and we worked through the question of whether this was a real event, not knowing whether there had been a blind injection or not. The question we were supposed to answer as the detection committee was: Was it real? We were not trying to distinguish between a blind injection and a real signal, but to determine whether there was something was real there and not caused by noise or any environmental effect. And I think we were successful. We actually came to the conclusion that it was real, and indeed it was real; it was just something *we* had injected, not Nature.

But it was a big job to do, time-consuming and stressful. And so after the Big Dog deliberations, I asked to be relieved as chair of the detection committee. Peter Saulson took it over and served as chair during the time when Advanced LIGO was being built and the early commissioning. Then just a few months before we were scheduled to have the first run with the Advanced LIGO detectors, Peter had asked Gaby if he could step down from that position. And Gaby came back to me and said, Could you take it over again?

ASPATURIAN: Gaby being?

WHITCOMB: Gabriela Gonzalez. She was the spokesperson for the LIGO Scientific Collaboration during the time Peter had been LIGO co-chair of the joint detection committee. She asked if I would co-chair the committee again, and my initial reaction was no. Then she leaned on me a little harder, and I finally said yes, but only under the condition that we wouldn't have meetings unless there was actually something to do. And since I knew I was retiring at just about the time that this run was starting, I thought, "Well, it's a pretty cushy job; I will have this nice, fancy title and everything, and I can pretend that I am actually part of this whole business, and I won't actually have to do any work," because I'd told Gaby we won't be meeting until there is actually an event. And so, you know, basically the day before the run was supposed to start and the day before I retired, this event happens.

ASPATURIAN: This is the universe saying to you, "Not so fast."

WHITCOMB: [Laughter] So that's why I was telling my wife as we were walking, "This cushy retirement that I'm supposed to be having is going to be a little bit busy."

ASPATURIAN: What were your thoughts when you first noticed this reading? Did you think that there was a possibility this might be an actual event, or did you just bring to it a general skepticism?

WHITCOMB: Oh I was absolutely convinced it was a real event.

ASPATURIAN: Right then and there, in the middle of the night?

WHITCOMB: Yes. Absolutely. And so the challenge for me over the next several months was to be the skeptic I was supposed to be and to force myself to ask the hard questions.

I was absolutely convinced it was real, but it was my job to be skeptical, and so I did my job. And we were very skeptical. I think we were extremely thorough and rigorous in evaluating this.

ASPATURIAN: When you first saw this event, and it came to you as a perception that it was genuine, how did you feel? A project to which you had devoted close to thirty years. One day before you were going to retire!

WHITCOMB: It was fantastic. [Laughter]

It was really exciting, and the fact that it was a binary black hole, not the mundane neutron star binaries that people thought would be our first source, was even better, because it's new—and different from what people were expecting. It was extraordinarily exciting.

ASPATURIAN: How much information were you able to glean just from this initial reading? What did you know, or what did you think you knew?

WHITCOMB: Even at that very early stage, we knew that it was a very massive system, just from the knowledge of which template had gone off. It eventually turned out to have a total mass of about sixty solar masses. We didn't know at that time that it was going to be twenty-nine and thirty-six solar masses, but we knew the total mass was large and that the separate masses were not too far from equal. We knew the time difference between Livingston and Hanford [Livingstone, Louisiana, and Hanford, Washington, sites of the two American LIGO detectors], so we knew the source was in the southern sky someplace. So we already had a lot of information about it.

There were a lot of people who in the very early going were surprised that the signal was so perfect and thought that must mean it was another blind injection (like the Big Dog had been), but I knew that it wasn't; and the reason was, we hadn't started the run officially. The run was supposed to officially start on that Monday, I think, and we hadn't started it. We'd actually postponed it for a week, and part of the reason we had postponed was because the blind injection hardware hadn't been tested.

ASPATURIAN: So you knew it could not possibly be a blind injection?

WHITCOMB: I knew it couldn't possibly be a blind injection. Well, I couldn't know positively—I knew that it was highly unlikely to be a blind injection. There was also the question of could it be a malicious injection? Somebody who was being dishonest and trying to embarrass the collaboration. That was a little harder to disprove, although I think we did a very good job of that in the end, but I also had personal faith in the people around the project. There's a very limited number of people who could possibly have done that, and I know most of them, and I have a lot of faith in their integrity.

ASPATURIAN: How widely was this news dispersed among the LIGO collaboration initially in that first week?

WHITCOMB: It was shared with everyone in the LIGO and Virgo collaborations.

ASPATURIAN: So about a thousand people were in on this?

WHITCOMB: Yes. You know, the first announcements were going out on some of the email lists, and the typical circulation of some of these email lists, like the data analysis council, the CCB [Compact Coalescing Binary] group, and the burst group. Each of those has email distributions of more than 100 or 150 people, so already just within that community we're already getting a pretty wide distribution. But Gaby sent out an email to the entire LIGO Scientific Collaboration [LSC] within a couple of days, in part to let people know, but in large part to remind people that we have confidentiality rules about keeping this to ourselves.

ASPATURIAN: Confidentiality among a group of 1,000-plus is tricky. But I wonder, were you stunned or surprised a) that this signal was so powerful, meaning it was from what had been considered a less likely source, and b) that it came even before you had really embarked on this advanced run? Both the timing of this event and its nature—what was your feeling about that?

WHITCOMB: Well, certainly the timing was a bit of a surprise, that it came so quickly, but I knew that the detectors were working properly, and we were in what was called an engineering run. All an engineering run is is the run-up to a science run. You have a little bit more latitude to play with things and make changes. You're still doing some calibrations and some testing, but the detectors are fundamentally working. So that part didn't bother me so much, except from an optics point of view: I was concerned about how the broader scientific community would react to the fact that this had taken place in an engineering run rather than in a science run, and that *they* would make a bigger deal of it.

I'd actually thought that this might well be the first kind of source that we would see—a black hole binary system. We have pretty good knowledge of how many neutron star systems there are, at least within a factor of ten in either direction. Binary black-hole systems we didn't have such a good numbers on—that is, the number of such systems—and their parameters, like masses, were much more uncertain. At the optimistic end, the thinking was that they would actually be much more common as gravitational-wave sources than the neutron-star systems. So in some sense it would have been surprising to see a neutron-star system that quickly, because we actually knew how frequently we should expect to see them. In that sense, seeing a black-hole system was less surprising, because we didn't have the same expectation about what the rate would be. So that was—it was quite exciting actually.

ASPATURIAN: I can imagine. Whom did you talk to initially about this, and what were your first conversations about?

WHITCOMB: I actually don't remember. It would have been Dave [David Reitze, executive director of the LIGO project] and Albert [Lazzarini, LIGO deputy director] downstairs and then starting to swap emails back and forth with the people on the detection committee and so on.

ASPATURIAN: How about here at Caltech? Where did the news go? The PMA [physics, mathematics, and astronomy] division chair, the president? Did you talk to Robbie

[Rochus] Vogt [Avery Distinguished Service Professor and Professor of Physics, emeritus; Caltech provost 1983–1987] for example? Were these individuals in the loop?

WHITCOMB: In principle, none of those people should have gotten any hint about this, because they're not part of the collaboration and so on. Until we actually had some reasonable confidence, it would have been inappropriate to have shared that with any of them. I believe that Dave Reitze did inform the division chair, Fiona Harrison [Rosen Professor of Physics and Astronomy], within a week or so after the event. Again, swearing her to secrecy. But other than that, no, we weren't supposed to talk to anyone at Caltech, and I didn't, at least.

ASPATURIAN: Apparently there were inklings and intimations that this had happened. Did some of the news leak out? As I said, a thousand people keeping a secret is a monumental undertaking.

WHITCOMB: Yes, certainly some people did let things slip. It's a big secret, and it would be hard to imagine really keeping it secret very long, but the hope is that the people who are told treat the information with the same kind of respect as the collaboration does. That didn't happen. There were leaks within a week. There were people on Twitter saying LIGO may have seen something. The fact that we had this blind injection process, and that it was known about outside the LIGO collaboration, gave some doubt to the outside people who had heard about a possible detection.

ASPATURIAN: You could use it as a bit of a decoy.

WHITCOMB: Yes. So even though we didn't explicitly say something about it possibly being a blind injection, you would see other people's comments online about, "Well, of course, this could be a blind injection." I knew that it wasn't a blind injection, but I wasn't about to say anything.

ASPATURIAN: It gave you some cover, in other words.

WHITCOMB: That whole blind injection process kept the lid on things a lot longer than I think it would have otherwise. That was not the main purpose behind the blind injections by any means, but it was a very positive secondary outcome.

ASPATURIAN: So what were your first steps after the news got out within the collaboration? Did your team swing into action to investigate thoroughly all possible alternatives to this being an actual event?

WHITCOMB: We did, although the detection committee didn't come in immediately. Our job was to review what other people were doing, and so we needed to give them some time. The other thing is, I was off on a week's vacation visiting my mother, and she knew I'd just retired, and yet— I can't be sitting in her apartment on my laptop for eight straight hours every day doing this kind of work without some good explanation for her. [Laughter]

ASPATURIAN: This is how one retires at Caltech. [Laughter]

WHITCOMB: Right. So there was a bit of planning for the future. I might have set up a Doodle poll [online scheduling system] for this detection committee to figure out when in the world we could actually start meeting. But our work wasn't really going to start until a couple of weeks after the event. The first thing we did—there's a group within the collaboration called the detector characterization group, and their job is to use a whole suite of tools to look at the data and try to determine whether the detector is operating normally and are there any other known possible causes for this signal. They go back and they look at magnetometers, and they look at seismometers and all of the other auxiliary sensors during that period of time. They look at the data to characterize how glitchy it is—are there jumps and spikes and peaks occurring at an abnormal rate? Is there any evidence that the detectors were not working correctly? They scoured the logs of the people on site and asked was there anyone on site who shouldn't have been there.

So we started doing some of those kinds of checks. The first thing that the detection committee does is take a very careful look at all of the detector characterization stuff. The people who are doing the data analysis for the actual gravitational-wave channel and pulling out the masses and all that kind of information need a little time before they have any results to check, whereas the detector characterization investigations are often much more compartmentalized.

So there was a big checklist of probably close to a hundred of those additional investigations that this detector characterization group had agreed to do, and so they assigned people from that group to go off and do these things. They were recording them in an online spreadsheet, and whenever somebody checked one of these off and said, "I've completed that investigation," someone from the detection committee was assigned to go in and look over their result: Did they do it thoroughly; was there anything in what the person from the detector characterization group had found that would cause a red flag—and then we would check it off as a sort of belt and suspenders sort of thing. So that was our first activity, and we kept that up for a couple of months.

ASPATURIAN: Did anything surface that threw anybody momentarily?

WHITCOMB: Not really. There was one thing, which is a little bit of a coincidence. One of the things that people look at is just what else was going on in the world—earthquakes and so on. In September, the third largest lightning strike in the world that month occurred within a couple of minutes or a few tens of seconds of our event. It happened to be in Africa and we saw no evidence of it on our magnetometers, but just the fact that it had occurred made us wonder, was there some other way that—

ASPATURIAN: Yes, you had to look at this.

WHITCOMB: And we did look, and there was nothing that could correlate. There were comparable lightning strikes that were closer to the detectors and showed up on magnetometers but didn't cause anything in our detectors. So we had pretty good evidence that even though there was this big lightning strike in Burkina Faso in Africa, it didn't actually have any plausible effect on our detectors.

ASPATURIAN: Did what had happened with the BICEP [Background Imaging of Cosmic Extragalactic Polarization] satellite influence your thinking at all? They, as of course you know, had made these extravagant claims for having detected gravitational waves from the inflationary epoch, and it all turned out to be a dust-riddled mistake. Was that in the back of your minds just as a cautionary tale?

WHITCOMB: Of course it was. And, you know, we wanted to be sure that everything had been looked at. So our detection committee spent what I think was an overly large amount of time dismissing the malicious injection claims—this idea that one of the people in the collaborations snuck in and cheated. We went to some very extreme measures. Within a couple of days after this event, one of the detection committee members went off and put these special seals on all of the electronics up at Hanford. The idea was that in case somebody had inserted something into the electronics that could cause this, and then wanted to go back and remove it to eliminate all traces, we would be able to detect if somebody had actually opened one of the electronics boxes and gone back in. And then, just right at the end of the run at the end of January, somebody went back through, broke the seals, and opened up the boxes to see if somebody put in an extra circuit board.

ASPATURIAN: Looking for any evidence of tampering.

WHITCOMB: Yes, any evidence of tampering. I kind of think that's a bit extreme in terms of the measures you take to try to find a dishonest scientist, but—

ASPATURIAN: It was so exciting, everyone wanted to be sure.

WHITCOMB: So we did all of that kind of stuff. We really tried to look at every possible way that we could be fooling ourselves.

ASPATURIAN: Were you still looking at the first event when the second event was detected in December?

WHITCOMB: Yes.

ASPATURIAN: Did that come as kind of a reassurance? It was another black-hole collision, I believe—smaller.

WHITCOMB: It was another black-hole event, with smaller black holes, and for me that made no difference. To my mind, we had such a perfect first event; and we had done a perfect job—an extraordinarily good job—of vetting it and verifying that it was real. I have this feeling that if you believe in your instrument, then you have to believe the data, and if this is what you measure, then this is what you measure. There were other people who really were much more skeptical, and that second event made a lot of difference to them; even some people on the detection committee who said, "Gee, I really rest a lot easier now that we have this second event."

ASPATURIAN: Who were some of the initial skeptics, do you recall?

WHITCOMB: I think Bruce Allen was one of the skeptics.

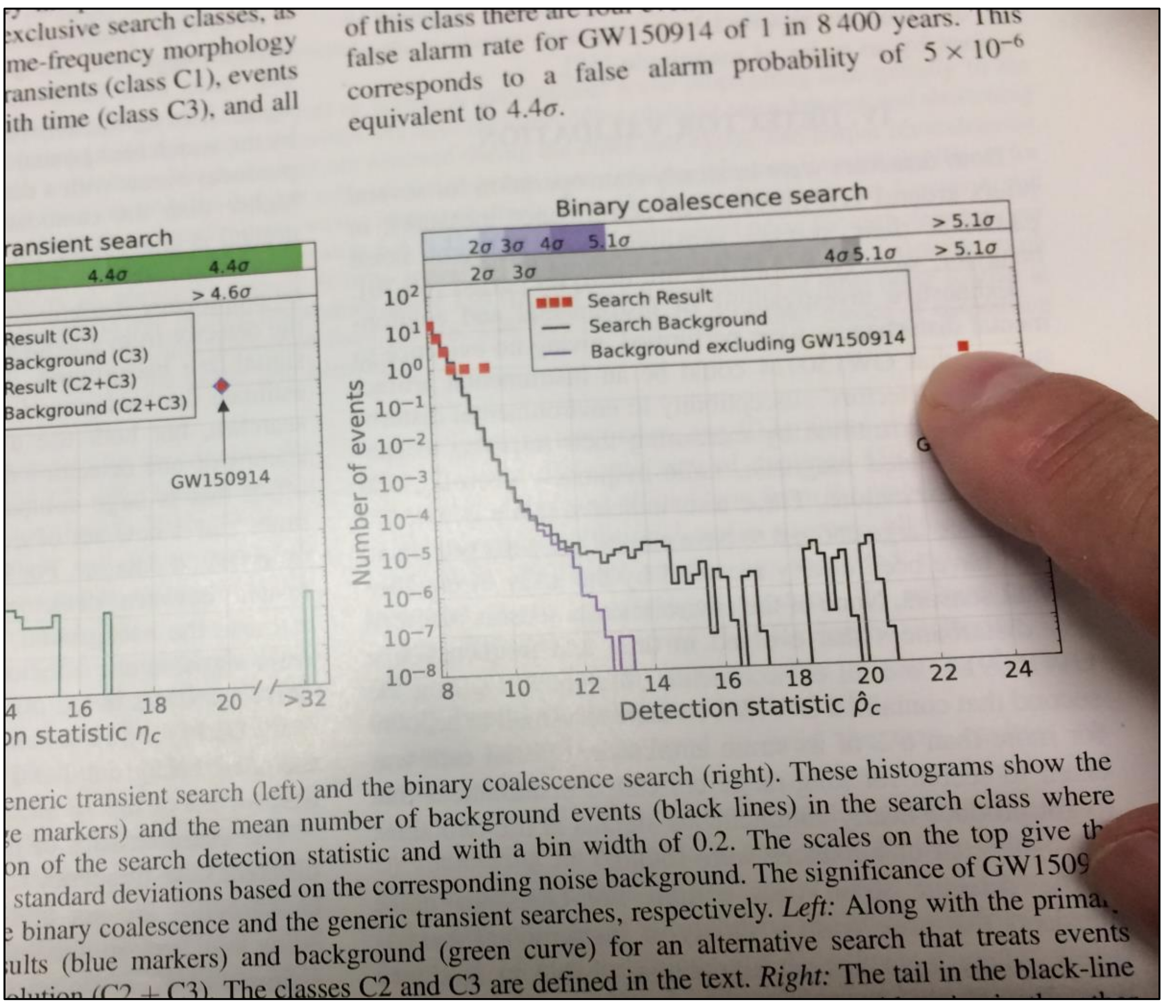
ASPATURIAN: He is where?

WHITCOMB: Bruce Allen is at the AEI, the Albert Einstein Institute in Hanover, Germany.

ASPATURIAN: Is he a part of the LIGO team also?

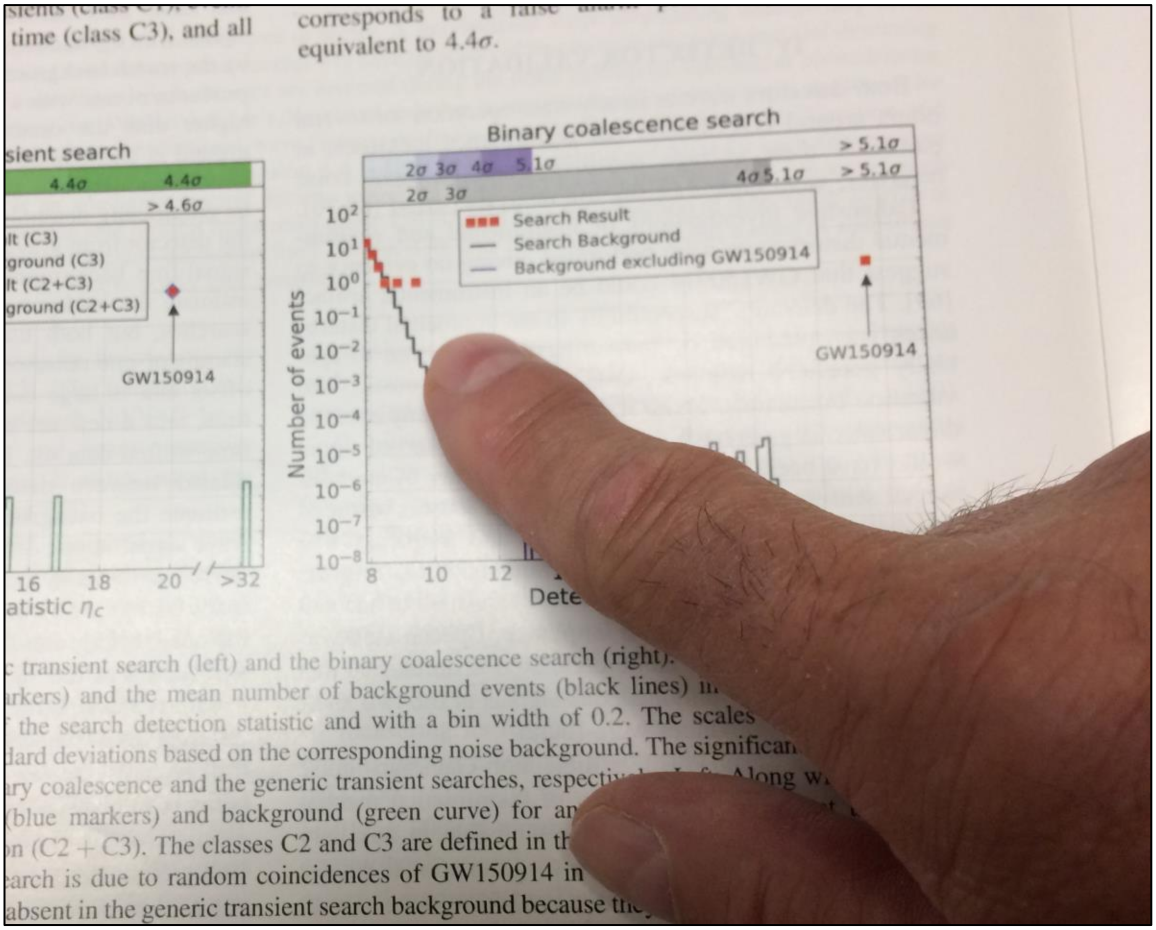
WHITCOMB: He's a part of the GEO collaboration [a German-British gravitational-wave collaboration] and so has his membership in the LSC by that means. His expertise is large-scale data analysis, the computing to do this kind of thing. He's not an experimenter; he has much more understanding of the theory of general relativity than I do, and so he was, I think, somewhat skeptical that maybe the detectors weren't really doing what we thought they were doing. So he was much reassured by that second event. Rai Weiss was, as well. I think Rai always felt a little bit awkward about having just a single event. He had this feeling that if it's real, it ought to be reproducible. Well you can't quite reproduce it, but seeing a second event surely makes it a lot easier to have confidence, and by the time we actually made our announcement, we'd gone far enough on the checks of the second event to know that it was also legitimate. I think the thing that many people in the collaboration were shocked at with this first event was how clearly it showed up above the noise. I think they were expecting something that was just going to *barely* be visible above the noise, and that there would be a lot of wringing of hands and trying to figure out whether this is sticking up far enough above the noise to actually be significant. [Bringing out a copy of the detection paper, "Observation of Gravitational Waves from a Binary Black Hole Merger": https://journals.aps.org/prl/pdf/10.1103/PhysRevLett.116.061102]

The thing that people were a little bit hesitant about in this first one—now I'm showing you a picture from our detection paper—is that our first event is *here*.



Way above the background [solid line].

Most people were expecting that they would be trying to tease out of the data whether something over in *this* region would be far enough above the background to be a real



event, not seeing something way out here that stood out so clearly.

ASPATURIAN: It wasn't troubled by noise much at all, looking at this schematic here.

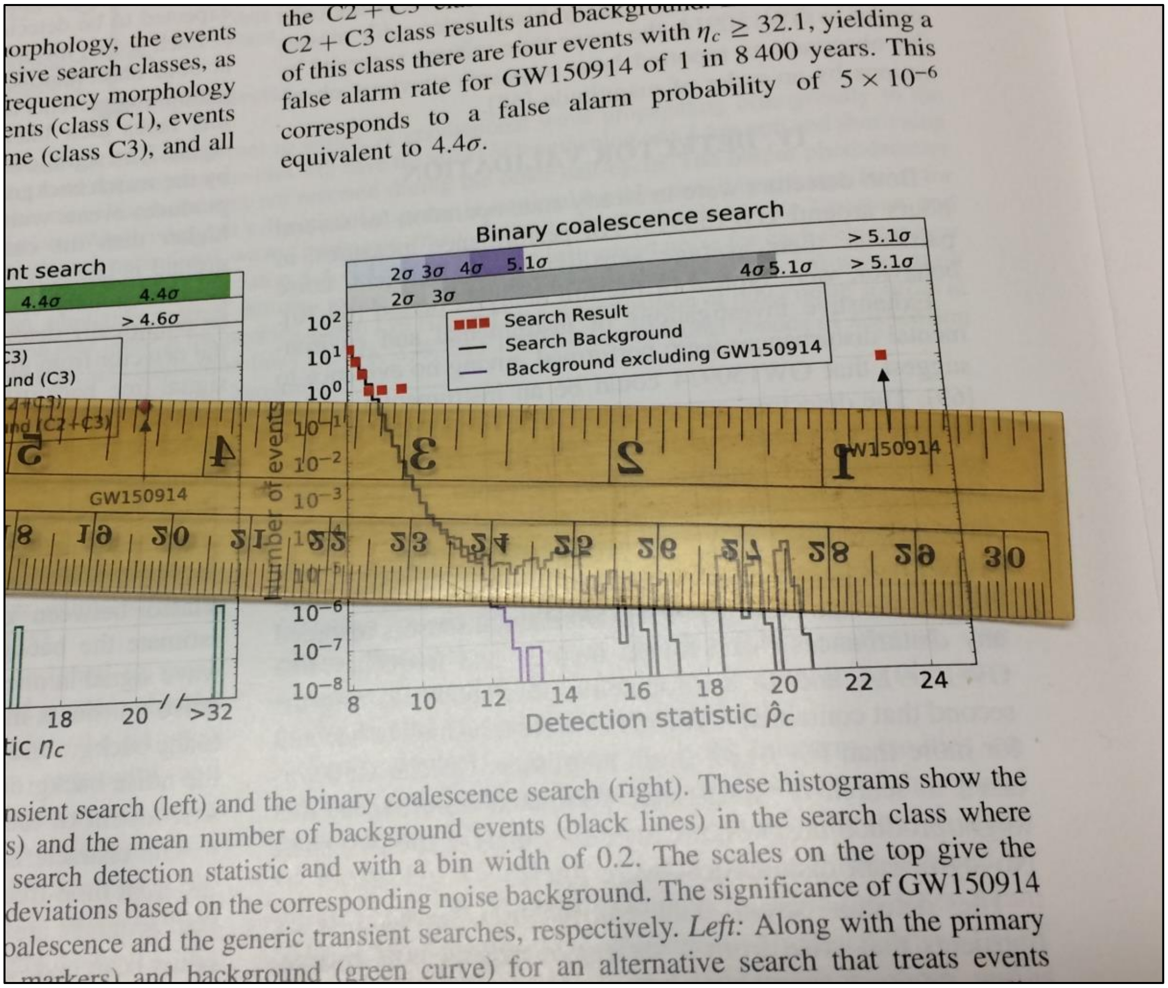
WHITCOMB: Exactly. Now I had actually come to a realization before we even started the run. There was a collaboration meeting in Budapest the first week in September [2015]. I held a meeting of the detection committee at that time, and we'd talked about this question of whether we were going to be looking for something in this noisy region or looking at a strong signal. I had told them that I thought if we had a first detection during this run, there was a 30 percent chance that it's going to be kind of over in this region—

ASPATURIAN: In the noisy domain, in other words.

WHITCOMB: In the noisy domain. And a two-thirds chance that it's going to be out here and is so clear that it sticks out like a sore thumb.

ASPATURIAN: You must have looked prophetic in retrospect, but what made you think that?

WHITCOMB: I'd come to a realization based on our experience with the initial LIGO detector that if you actually ask, "If we got a whole bunch of detections, what would the distribution of those detections look like?" that the distribution looks something like the slope of this ruler.



It’s very flat. It falls a little bit with signal strength but not very fast. Over this kind of range it would be something like that, OK?

ASPATURIAN: I see what you’re saying.

WHITCOMB: I’m not the first person to have noticed that. Bernard Schutz [principal investigator for data analysis at GEO] actually had the basic idea in some work that he had done, and then I picked up on it and maybe extended it a little bit. So I actually came to that detection committee meeting with that point of view—that our challenge was not going to be dealing with something that was kind of weak and just barely sticking out above the noise. Our job was going to be trying to figure out whether there was any other possibility for this whopping big signal.

ASPATURIAN: I'm going to go back for a moment to the quote of Barry's that we started with—when he predicted that it would be five or ten years before LIGO detected something. Was it a surprise to everybody, or to you, that this showed up so quickly?

WHITCOMB: A bit of a surprise, maybe, but not so much a huge surprise, at least to me. I was pleased with it. I thought it would happen in probably our second run, which is the one we're engaged in now. So I thought it would be a year away.

ASPATURIAN: If you were fairly confident that it might be within a year to two year's time, why did you decide to retire?

WHITCOMB: Well, a variety of reasons. One, I was getting kind of fed up with the bureaucracy of a large collaboration.

ASPATURIAN: Not what you started with, that's for sure.

WHITCOMB: It's certainly not what I started with, and so I was tiring of that. I had gotten caught up in management and felt somewhat disconnected from the detectors. That history we'll probably discuss at some point. [S*ee Sessions Four and Five*]

ASPATURIAN: Yes.

WHITCOMB: So I was just feeling like I was mostly one of the hangers-on at this point. And I was at a place where financially it didn't matter much whether I continued to work or not, and so my plan had been just to retire, and then if anybody asked me to do something, the only criteria about whether or not I did it was whether it was satisfying enough that I would want to do for free.

ASPATURIAN: What do you recall about the stages of preparing for the announcement, and what was your role in this? Everyone must have known what excitement this was going to cause when the official word finally got out.

WHITCOMB: Yes. We all knew that it was going to be a big announcement. I think it turned out to be even bigger than any of us expected. I thought, "Gee, we might get an article that would start on the front page of the *New York Times* down in the corner and then be continued on page eight," and that would be really wonderful, and probably the local newspaper would carry it as a big deal; but I did not expect quite the big pomp and circumstance. I didn't have very much to do with the actual announcement, which was handled primarily by Dave and Gaby and Kip and Rai. They got courses in how to deal with the press and how to dress and so on. There were a bunch of us—sort of the second tier group of people—who also got the short course on that kind of thing, but since we were mostly expected to be in the background we didn't have to go through the grooming—

ASPATURIAN: Quite so stringently?

WHITCOMB: Yes. Exactly. Anyway, it was very exciting, and it was quite fun to see the attention that it got, and the real excitement that people had about it. That was quite fun.

ASPATURIAN: It was very thrilling.

WHITCOMB: I think that's actually been undervalued by Congress and so on. I actually went to the big announcement in Washington D.C. I hadn't really planned on doing it but—

ASPATURIAN: This was the NSF [National Science Foundation] announcement with France Cordova [NSF director]?

WHITCOMB: Yes. I was asked to go to that press thing and then to spend that afternoon and then the following day doing some visits to Congressional staff. So April Burke and her crew—April is the lobbyist who was originally hired by Robbie back in the day to help guide LIGO through that whole Congressional approval process—had arranged for a bunch of visits to Congressional staff for the different science committees and also the Congressional staff for the representatives and even a senator or two from Washington and Louisiana. David Shoemaker and I were assigned the job of being the captive physicists to go around and talk to these staff and thank them for their support, and so that's what I did.

The thing that I was disappointed in was that although all of the Congressional staff were quite positive about LIGO, they were all focused much more on the technology, the training of scientists, and so on. I kept trying to bring up the value of the discovery in exciting students—exciting people—about science, getting the public excited about it so that children get encouraged to be curious about this and go into science. And that received really absolutely no traction whatsoever in Congress.

ASPATURIAN: No real interest in the fundamental research aspect of it at all?

WHITCOMB: Or the excitement about how it inspires and excites kids.

ASPATURIAN: Have you been to Congress before? Was this a change? I think things were quite different, say, twenty-five years ago.

WHITCOMB: I had been to Congress before, but not that often. I think there's always been a focus on the near-term practical aspects of research in Congress. There is a tendency to ask how many jobs will this project create for my district, that kind of thing. That whole excitement of students, that side of things, just never gets very much traction.

ASPATURIAN: There is this story, which for quite a while I thought might have been apocryphal, but Janna Levin tells it as well [in her account of the LIGO project, *Black Hole Blues*], of Robbie Vogt convincing this congressman or senator from—was it Louisiana?

WHITCOMB: Yes, Senator Bennett [John Bennett] Johnston.

ASPATURIAN: The two of them were evidently on the floor looking at pictures Robbie was drawing of the early universe. And that helped get the funding for LIGO going. But it doesn't sound like you encountered any of that in this visit.

WHITCOMB: Not really, no.

ASPATURIAN: Speaking of Robbie, did you talk to him about this? What was his reaction? He was noticeably absent from all the coverage, and yet he did play quite a role in getting this off the ground.

WHITCOMB: Robbie hasn't really spoken to me for twenty years, so no, I haven't spoken to him.

ASPATURIAN: I didn't know that. OK, we may get to that. [*Session Three*]

WHITCOMB: It's possible we might get to that, yes. [Laughter]

ASPATURIAN: I mean he was an advisor for you as an undergraduate, and he brought you back onto the project.

WHITCOMB: Yes.

ASPATURIAN: Something happened obviously.

WHITCOMB: There's a history there, but this is probably not quite the right place in this session.

ASPATURIAN: No, it just occurred to me, he was *not* mentioned, I think, or quoted in any way.

WHITCOMB: Yes. He still has an office down the hallway here, but he hasn't spoken to me since he left LIGO in 1998 or so.

ASPATURIAN: That's very sad.

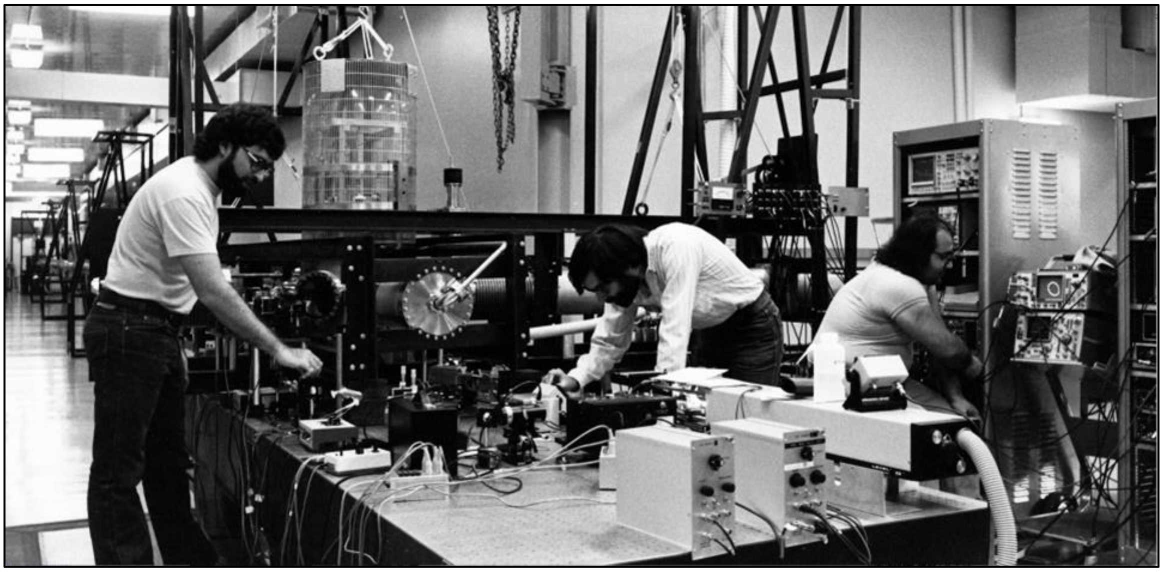
WHITCOMB: It is. It's extraordinarily sad.

ASPATURIAN: For you personally, what was the most meaningful or significant aspect of this whole detection? I mean I found it thrilling, and I haven't spent thirty years working on it.

WHITCOMB: This isn't going to quite answer your question, but maybe it'll give a bit of a feel. I'll have to say that the first emotion that I had on seeing this was relief—much more than excitement or joy. You know, I'm one of the people who is almost as much as anyone to blame for dragging the country down this path of investing a billion dollars in this project. OK, there are others who have more of a role in that, but if you list the people in order of their responsibility for expending this billion dollars, I'm in the top one percent, certainly. I was even beginning to worry about whether it was the right thing to have done. If we had gone forward and the only things we had seen were the things that people expected to see, or the events were rare enough to say, "Gee, there's nothing exciting about general relativity; yeah we occasionally see a neutron star binary system, but—" that would for me have been a very, very big disappointment. So to see this, and to see it be this type of source that most people weren't expecting—a binary black hole, and one that was much more massive than what people had talked about for the most part—it was just such a relief to say, "Gosh, we really did see something exciting and new, and this money was not all spent in vain—the country didn't do this for no good reason." So actually that sense of relief was the first and most powerful emotion that I had.

ASPATURIAN: Anything else about the discovery?

WHITCOMB: No, I think that's everything.



Stan Whitcomb (center) bending over LIGO's prototype detector in 1985.

And 32 years later, at right, with LIGO director Dave Reitze (left) and LSC spokesman David Shoemaker at the 2017 Nobel ceremonies in Stockholm, where LIGO scientists Barry Barish, Kip Thorne, and Rainier Weiss received that year's Nobel Prize in Physics for the successful detection of gravitational waves.

STANLEY WHITCOMB

SESSION 2

May 8, 2017

ASPATURIAN: We are going to step back nearly three decades today and talk about your reentry into the LIGO project in—was it ’90 or ’91, specifically?

WHITCOMB: ’91.

ASPATURIAN: Did you have any hesitation about coming back when the offer was made to you? Via, I guess, Jocelyn Keene first and then Robbie Vogt? [For additional details on this period between 1991 and 1997, see the 1997 oral history interview with Stanley Whitcomb: http://oralhistories.library.caltech.edu/240/]

WHITCOMB: Well, the first contact was Jocelyn, who asked this very bizarre question—Would I be willing to talk to Robbie Vogt, or would I hang up immediately on him? And that was the extent of her involvement. I think she felt very uncomfortable about that question, and so when I was able to say, yes, I would be willing to talk with Robbie, she relayed that back to him and then got out of that conversation. [Laughter]

ASPATURIAN: Why would Robbie have thought you wouldn’t want to talk to him?

WHITCOMB: I had left Caltech on not very happy terms. I had realized that my career was going nowhere. I was an assistant professor, and I was not going to get tenure and would get flung out, so I could either let that happen and then have to figure out what to do, or I could figure out what to do and avoid the actual tenure denial.

ASPATURIAN: You talked a little about that in your earlier series of interviews. I guess some of the problems stemmed from Ron Drever’s [professor of physics, emeritus, d. 2017] management of the project; and also, from the fact that there wasn’t that much management of the project at that point.

WHITCOMB: Yes. Ron was absolutely— he had to control everything. I tried to negotiate some kind of division of responsibilities where I would either do the legwork for the initial detectors or do advanced R&D that would leave *him* free to do the initial detectors the way he wanted. But he was unwilling to let go of anything, so everything had to be under his control, and I knew that that was not going to give me a satisfactory tenure case. Kip [Thorne, Feynman Professor of Theoretical Physics, emeritus; 2017 Nobel laureate in physics] will sometimes dispute this and say I would have gotten tenure, but I think Kip sometimes overestimates his influence in that sort of situation. I saw enough to know that that wasn't going to happen.

ASPATURIAN: You felt you saw the handwriting on the wall.

WHITCOMB: Yes. I later saw some things that had been written about my renewal, and— it just wasn't going to happen.

ASPATURIAN: I understand. So to get back to my earlier question: Did you have any hesitation about coming back? It would not have been as a faculty member. You were coming back as a high-ranking staff member, but a staff member. How did you feel? What did you think?

WHITCOMB: It took me a while to feel comfortable with that. I really wanted to come back to the project, because I found the work was completely exciting—the mission of making such a precise measurement was just something captivating. I really wanted to be a part of that. But I also knew that in the time I had been there before, things were not going well, and I wanted to be sure that this time things would be better. I wanted to understand how Robbie saw the project: He was the project director; how did *he* see it? Would I be stepping back into the exact same situation of being completely subservient and dependent upon Ron, or was that going to be different?

So Robbie and I had a series of meetings—dinner meetings oftentimes that might go on for several hours; and there were about three or four of those before he finally made an offer and said, "Do you want to come back?" and I agreed to do it. So I came back as the deputy director, his deputy. That was something that—I think Rai was the person who really recognized that Robbie needed somebody he trusted in that role.

ASPATURIAN: This is Rai Weiss.

WHITCOMB: Rai Weiss. He saw that Robbie needed someone he trusted a lot that he could offload some things on to. I don't think Robbie totally trusted Rai, and I don't think he totally trusted Ron. He did trust Bill Althouse, who was the chief engineer, and there were some things that Bill Althouse could do very, very well, but Bill was reluctant to get involved in the science side of things. And so Rai, I think, was the person who had pushed Robbie rather hard to identify someone that he would feel comfortable with as a deputy, and I kind of fit that bill. I knew something about the project; I was enough younger than Robbie that I wasn't likely to challenge him, which I think was possibly an important feature as well. And I wasn't in a faculty position someplace else, so they didn't have to use a faculty position to lure me back.

ASPATURIAN: Aha.

WHITCOMB: So I said yes.

ASPATURIAN: What kind of environment did you find that you had stepped into when you got back? Of course it was now a joint project between Caltech and MIT, but there were many other things going on as well.

WHITCOMB: There were some significant problems. There had also been some great work that had been done.

ASPATURIAN: In your absence?

WHITCOMB: Yes. In '86, there had been a summer school at MIT, organized by Rai Weiss at the suggestion of NSF, that was really an attempt by the NSF to have a blue-ribbon panel look over the project and give advice about whether this was really something that NSF wanted to do. This was before Robbie came onto the project, and it was partly, maybe almost entirely, instigated and precipitated by Dick [Richard] Garwin, who was on the panel. I think Bob [Robert] Byer and Dan [Daniel] DeBra were on it; these were very significant players. Boyce McDaniel and Joe [Joseph] Taylor. Andy [Andrew] Sessler may have been a member of that panel. [Sessler and McDaniel co-chaired the panel. *–Ed.*] And the combined efforts of the MIT and the Caltech groups convinced this panel of really heavy hitters that, first of all, it was an exciting project from the point of view of physics and astrophysics, and, second, that the technology for reaching the sensitivity was achievable. It was not going to be easy, but it could be done. The panel report said that the project was going to be big and expensive, and it should be pursued.

They also, not intentionally or maybe partly intentionally, recommended a different management for the project. I looked at Rai's oral history [http://oralhistories.library.caltech.edu/183/], and he has claimed that he tried to get this panel to recommend some changes in LIGO's management. So maybe it was the panel's idea, and maybe it was Rai's—I don't know, but the panel did recommend changes. The panel came back and said this project absolutely has to have a single director, someone to run the project. It has to be somebody with the authority to do what the troika had been trying to—

ASPATURIAN: Just for the record, the "troika" was Kip Thorne, Ron Drever, and Rai Weiss.

WHITCOMB: Correct. And they wanted to do the management, but they also wanted to have a project manager who would do all of the dirty work of designing stuff and building it, and the troika would oversee the manager. Well, it was putting the manager in a subservient position where it was impossible to succeed.

ASPATURIAN: In an untenable situation there, yes.

WHITCOMB: They had a guy Ernie [Ernest] Franzgrote from JPL, who I think served in that role for a while. It wasn't very successful, and eventually he left. And as a result of this panel recommending the single director, Caltech came up with Robbie Vogt. MIT said, "Well of course Rai should do this," and that wasn't going to fly with Kip and Ron. And so ultimately Rai agreed to let Robbie do it.

Robbie had recognized that the two groups were not cooperating at all, and he tried to insist that the groups cooperate and so he set up small groups to address different kinds of questions—for example, what's the right size and shape for the mirrors? Well-defined questions. He would typically assign one scientist from MIT and one from Caltech to work on this together and give back a report to the group. And there were a large number of these studies, and over that period of time a lot of those questions got investigated, and real analyses were done. You find, looking at many of those reports now, that they produced good material, but more importantly the two groups actually started talking to each other and collaborating and started thinking of themselves as one single group. At the same time, Robbie brought in a group of engineers—people like Larry Jones, Fred Asiri, and Baude Moore—who really began to flesh out the engineering side of things. Rai adapted very well to this environment.

ASPATURIAN: So Rai and Robbie wound up working reasonably well together?

WHITCOMB: Yes—Rai had to stretch in order to do that. Robbie was a bit dictatorial and needed to be accommodated at times, and I think Rai had to do that. Reading Rai's oral history, he talks about some of the things he had to accept in order to keep peace between himself and Robbie, and if you know Robbie, that's probably the way that the accommodation had to go. But they were working reasonably well together; Ron not so well. I think in the beginning Ron expected that Robbie, being a Caltech faculty member, would support him on everything. And I think his notion was that Caltech had won, that Robbie was going to come in, implement all of Ron's ideas, and keep "those guys from MIT" from ruining things.

In the beginning Robbie made a lot of hard decisions that the troika had not been able to make, like the configuration of the interferometer, and would the interferometer be a Fabry–Pérot or a simple Michelson. How big would it be? Those were questions that the troika had never been able to agree on, and Robbie just would make those decisions. He would ask people what they knew about it, and maybe he would commission somebody to study it, and then he would make a decision. And once a decision was made, it was made. Ron bristled under that, as much as anything really, because he hated making any kind of final decision. Because any time you make a decision, you're closing off certain paths, and he didn't like to do that. He hated to do that. And so he bristled more and more at that and was trying to enlist allies to help him battle against Robbie.

ASPATURIAN: Within the project, within the campus?

WHITCOMB: Both, I think. And within the broader community. But he was not very successful within the LIGO team, because in fact most of the people in the LIGO group at Caltech felt like he hadn't been very supportive of them and they weren't very inclined to help him. Under Robbie's leadership, they actually saw real progress being made on many of these decisions that had gone unmade for years, and so I think in many ways they saw Robbie as being their champion for moving things forward.

ASPATURIAN: Was that your perspective at that time, or was this before you came in?

WHITCOMB: This was about the time I came in.

ASPATURIAN: So this is what you walked into.

WHITCOMB: So I walked into that. Robbie more or less had me manage the science team, and I tried to move things forward to the extent that we could. Sometimes that meant just ignoring things that Ron would say and trying to move meetings and decisions along, and I think we made some reasonable progress. But the pressures on Ron—I think he was feeling that he was being shut out of the project and he was right, because things were moving along, sometimes without even his acquiescence. Oftentimes he would give his input, but he had a hard time with it because he couldn't communicate anything to Rai. Rai wants to see things worked out in detail; he wants to see the mathematics behind something. Ron was terrible with that stuff. He saw everything in little pictures, and so he couldn't grasp how to communicate his ideas to Rai, and so I think he was feeling more and more puzzled about why he's not able to have more influence. The '89 proposal had gone in—

ASPATURIAN: This is to the NSF.

WHITCOMB: The NSF. It's a masterful proposal. That's not bragging, because I was not involved in it.

ASPATURIAN: That was before you came back, yes?

WHITCOMB: Before I came back. I look back at it; it's a masterful piece of work. The costs are maybe a bit of an underestimate, but not an outrageous one. It was an underestimate by 20 percent, maybe.

ASPATURIAN: I am smiling because these costs are often optimistically underestimated in these kinds of productions.

WHITCOMB: Yes. If you look at something like the SSC [Superconducting Super Collider], it escalated in cost by a factor of two or three. The LIGO proposal went in initially at $200 million, and in the end when Barry Barish finally finished building it, it was about $260 million. So the cost estimate was actually pretty good.

ASPATURIAN: And Robbie had basically quarterbacked this proposal.

WHITCOMB: Yes. And then the proposal was submitted. The NSF had been expecting much less money. They had a number that they got someplace—nobody quite knows where—of $180 million, and they had taken that to the National Science Board, so when the proposal actually was being prepared, they told Robbie, "You can't put in more than $180 million into the proposal." And Robbie said, "I won't submit it at $180 million." They finally came to an agreement that since the LIGO group had been getting $4 million a year, they would add in that $4 million for the five years that the proposal covered, so that this proposal was $180 million on top of what the group had been getting for R&D. And that brought it to the $200 million over five years. It was still quite a bit less than what Robbie wanted, but because the NSF had already picked out a number, they were committed to it.

So that sets a bit of the stage at the time I came back: Robbie was already battling with the NSF because they were squeezing him monetarily. Robbie believed that when he wrote that proposal he was proposing $200 million in 1989 dollars, and that whatever inflation might to do that figure by the mid-’90s he would get the inflation-adjustment on top of that. And the NSF firmly believed that they had gotten a proposal in then-year dollars and that any adjustment for inflation had already been included. So they were battling over those kinds of issues. It’s my belief that at the time that proposal went in, the NSF didn’t have much experience dealing with inflation and didn’t have particular standards about this sort of thing. So there were issues—kind of well-documented issues—that Robbie was not entirely at ease about with the NSF. They were not always on the same page; there were some tensions there.

More or less, the first thing I got involved with was the site selection—Bill Althouse and I led that site-evaluation team. I think Rai gets much of the site-selection stuff wrong in his oral history, but people remember things differently. The NSF said, “submit your proposal,” and then at some point after they had agreed to the proposal they said, “Now you need to start a site selection process,” and they gave us a little bit of money to do the site selection. This is really rambling—

ASPATURIAN: Are you taking that from my expression? It’s like listening to a novel. It’s not rambling at all.

WHITCOMB: So the rules said we had to issue a solicitation in the *Commerce Business Daily*, which is where the government advertises for anything it wants to buy. It’s where the government publishes if they need to buy toilet paper for the restrooms in the National Parks Service. If they need to buy a destroyer for the Navy, they publish “sell us a destroyer” in the *Commerce Business Daily*. So we put in an advertisement saying, “If anybody has a LIGO site that they want to offer up to us, let us know and we’ll evaluate it." We got eighteen proposals with nineteen different sites in seventeen states, and we quickly got into a situation where we had to figure out how do you evaluate these things? Because you need to do an individual site evaluation but you also need to evaluate sites as a network, as a pair. So after we told NSF, "Look this is really difficult, and here's what we're doing," they came back and told us that what they wanted was an unranked group of best pairs of sites. And that's *all* they told us. Rich [Richard] Isaacson [then NSF's LIGO Program Officer] told us later that they believed they were going to get just two or three pairs. But they didn't tell us what the final number should be.

So first we actually evaluated how suitable the individual sites were. For example, there was a California site near Palmdale that ran right alongside the San Andreas Fault. As an individual site, that made it just awful. It was going to be expensive and dangerous, and so we excluded that. Then we took the pairs of the best sites and started evaluating them. We came up with seven best pairs, wrote a report, and gave that to the NSF. And they said, "We want an unranked set of best pairs and we will choose one," and they did. So that's what I was involved in when I first came back to LIGO.

So I wasn't working terribly closely with Ron in those days—he was doing some things in the 40–meter lab. And then once we got the sites selected, we started going into the design of the vacuum systems and trying to get companies under contract and doing the site environmental studies. So I was involved in a lot of that kind of stuff. I was involved in trying to write a specification for the vacuum system design, which needed to have an interferometer design because you want to fit the vacuum system around the interferometer, not the other way around. Well, there were a lot of things that were still not decided there. We pulled together and actually were working on the design requirements for the vacuum system.

ASPATURIAN: When you say "we," this is a joint effort between Caltech and MIT? You and your staff?

WHITCOMB: Yes. And the engineering staff, which was all here.

ASPATURIAN: I see. But the scientific collaboration between Caltech and MIT was very much a part of this.

WHITCOMB: Yes. And Ron was increasingly uncomfortable with things. He was going around trying to slow things down.

ASPATURIAN: I'd like to interpose. Ron Drever is generally acknowledged to be a great intuitive scientist. What were your feelings or your thoughts as you watched this happening, knowing what his gifts were and also his limitations?

WHITCOMB: I thought that we had largely gotten most of what we needed from Ron, at least for the initial detectors, and that most of the things that he was very concerned about at the time were non-issues or actually in many cases involved resisting progress that was needed. So I saw him being, to be honest, more of a drag than a help.

ASPATURIAN: The key insights that he had made were there, but he had little more to offer?

WHITCOMB: Because he often didn't know how to translate his ideas to a scale that would be suitable for this. I am not sure he ever *really* wanted to build a large interferometer. I think he always hoped that he'd be able to detect gravitational waves with something on the scale of the 40-meter or something like that. I don't know—it's hard to know. But I don't think he ever was comfortable with a big facility.

ASPATURIAN: Visionaries often can't see beyond their own vision. That's their strength and their weakness, I guess.

WHITCOMB: Yes.

ASPATURIAN: And so— I'm sorry, go on.

WHITCOMB: So Ron was growing increasingly restive, and Robbie was becoming increasingly impatient with him. And they were having shouting matches. Mostly Robbie shouting at Ron, and Ron acting like he doesn't understand. But then Ron was going around and off to conferences saying LIGO's not going to work, LIGO's not going to work.

ASPATURIAN: This really happened?

WHITCOMB: Yes. We had reports from people who had been at these conferences telling us that Ron was there and saying it's not going to work. So he was in many ways—I think he was trying to slow things down, because he felt he was losing control over the project, and feeling, how can it possibly succeed if I lose control? I don't know that he was being malicious; I think he honestly believed it couldn't succeed without his participation, and so he had to slow it down so that he could maintain control. And that's what broke the camel's back then. So it was a pretty restive time.

ASPATURIAN: When you talk about breaking the camel's back—

WHITCOMB: Robbie went to Gerry Neugebauer [Millikan Professor of Physics, emeritus, d. 2014], who was the [PMA] division chair and asked that Ron be removed from the project.

ASPATURIAN: Did you know he was going to do this? Had he consulted with you or with anybody before he took this step?

WHITCOMB: He might have told me that he was going to do it; I don't remember. He certainly didn't ask my permission. He might have asked did I see any reason why we had to keep Ron, and to be honest I didn't. I thought keeping Ron was probably more destructive than it was worth at that point. And so Gerry removed Ron from the project. What did that mean? He probably pulled away that fraction of Ron's salary that was being charged to the LIGO grant and charged it back to the division. Ron no longer had a guaranteed access to the budget for travelling to conferences. Ron's office was just across the way here [i.e., from the office in Norman Bridge Laboratory of Physics where these interviews were held], and I think Gerry put in place a plan to move Ron someplace else. There was the famous key incident: Robbie left a message for Ron saying, “We’re going to re-key your office.” All the offices had been keyed the same way, so they were going to re-key Ron’s office so that, Robbie said, he couldn’t be accused of getting into Ron’s office. But I think it was just as much to provide separation.

ASPATURIAN: I gather there was an incident where a wall or a door was bricked up, or something like this as well?

WHITCOMB: Oh that had happened long before. Ron had originally had the entrance to his office through a door that meant he came in through the secretary’s office, but the secretary found that she was all the time being bugged by Ron. She was really the secretary for the entire group, so Robbie had had that door bricked up, I believe, to give the secretary a little bit more space so she could handle work for other people as well. And probably also to make it clear to Ron that she wasn’t his personal secretary, she was the group secretary.

ASPATURIAN: Now you were not leading the project, but you were Robbie’s deputy and you knew a lot about Caltech, having been here as an undergrad. So the culture must have been pretty familiar to you. As you watched Robbie doing this, not so much disassociating Ron from the project, but some of the other things he did— the rekeying and so forth—did you feel he was going about this the right way? Did you imagine that there might be repercussions, as there eventually were, based on what you saw happening and your knowledge of how the campus operated? It was a long time ago, I know, but—

WHITCOMB: A long time ago. I knew that it was certainly a very tense time, and those were harsh actions to be taking, but that there wasn’t going to be, under any circumstances, an easy way to separate Ron from the project. You know, Ron was just not the kind of person you could sit down and discuss something like this with; that would have been impossible.

ASPATURIAN: Subtlety was not part of his make-up.

WHITCOMB: No. I don't think there was an easier way to divorce Ron from the project. I don't think there was a way. Some people think that Ron should have been eased aside and given some other kind of forward-looking responsibilities. It wouldn't have worked. Ron wouldn't have agreed to that, and he would keep niggling until something like this had to happen.

ASPATURIAN: As all of this went forward, was any work getting done on the project, or was it such a huge distraction that it amounted to causing a big delay?

WHITCOMB: Well it was causing a delay. It *did* cause a huge delay. Some work was getting done; but Robbie would get into a funk and then oftentimes he would call me and Bill Althouse into his office, and we would spend an entire afternoon listening to Robbie rant about things.

ASPATURIAN: So this all took a toll on Robbie as well.

WHITCOMB: It took an *immense* toll on him. He was— Ron eventually filed a complaint with the academic freedom and tenure committee, and they had a series of hearings. Robbie felt like he needed to respond to all of Ron's charges, so he got everybody on the project to write down things about their interactions with Ron. At the same time Ron was busy wasting all of *his* time doing that same sort of thing, writing down everything wrong that anybody had ever done to him. As was Peter Goldreich [DuBridge Professor of Astrophysics and Planetary Science, emeritus, who was a vocal advocate for Drever in what became known as the "Drever Affair" –*Ed*.] and so on. So it was a huge amount of wasted effort on campus over this.

ASPATURIAN: Did you look around at this time and think, "Maybe I should consider leaving again"? You'd already been down one sort of blind alley with LIGO.

WHITCOMB: You know, I still believed. I thought we were still making some significant progress. Look, during that period of time, we got the environmental impact study done on the site up in Washington and then in Louisiana. We'd made progress on the design of the vacuum system—we had contracts in place or under way. The work on the 40-meter was still continuing, and we were improving things, and we were hiring people. We'd hired somebody to lead the data analysis and data acquisition system; we hired some new scientists to try to grow the group; and we hired a deputy chief engineer from JPL [Jet Propulsion Laboratory], Gerry Stapfer.

ASPATURIAN: When you said "we," with so much of Robbie's attention diverted, did a lot of this fall on you as his deputy? Hiring, organizing, monitoring? Overseeing?

WHITCOMB: Yes, a lot of it did, but at the same time I was feeling a lot of pressure because Robbie wanted to talk about *everything*, so yes. There were some things that he was not willing to let go of. There were things going on—all the stuff on campus, and some of the issues with the NSF were coming to a head at the same time. The NSF was unhappy that we weren't making faster progress on some things. Robbie would tell them, "We're fighting this battle on campus," and NSF would say, "Well that's not our problem. That's Caltech's problem, and we want to know why you aren't making better progress on things." Robbie would get into these terrible funks, and no one quite knew how to deal with that.

ASPATURIAN: Had you been aware of this aspect of his personality when you were working with him as an undergraduate, or did this all come as news?

WHITCOMB: No, I knew about it. I had seen glimpses of it as an undergraduate, and when I was an assistant professor there was the whole blow-up about Murph [Marvin L. Goldberger, Caltech president 1978–1987] and the Arroyo Center, and you could see Robbie's strengths and weaknesses there.

ASPATURIAN: So you were dealing with this again, in real time.

WHITCOMB: Everhart [Thomas E. Everhart, Caltech president, 1987–1997] had an idea to bring Ned [Edwin] Goldwasser in from the University of Illinois. Ned had been the first deputy director of Fermilab and had worked with Robbie on some of the oversight committees at the national labs. He was somebody Robbie respected whom Everhart knew. He was brought in to sort of calm Robbie down; advise, mentor, soothe, handhold, analyze, and so on. I got to know Ned. He was coming down and spending, you know, every other week out here at Caltech. He was a delightful guy during that time. He died recently.

ASPATURIAN: Did he live long enough to see the LIGO discovery?

WHITCOMB: He did. His death was only within the last few months [December 2016]. And I have to say that one of the things that was quite interesting in his obituary is that apparently he pointed with pride to his time with LIGO. He's one of the two physicists in the world that I respect most as a person.

ASPATURIAN: The other one?

WHITCOMB: The other one is my advisor [at the University of Chicago] Roger Hildebrand. But Ned was fantastic. Anyway, so people were trying to figure out how to get things moving.

ASPATURIAN: I'm sure.

WHITCOMB: But we were actually getting some stuff done. Barry in his oral history doesn't think we were making much progress, but we were actually doing some things. Not progress at the level that you might expect, but we were still getting some things done.

ASPATURIAN: I have a quote here from Janna Levin's book in which she says you were the deputy director, aide, ambassador, and part therapist for Robbie Vogt during this period.

WHITCOMB: That's probably about right.

ASPATURIAN: You would say that's that a fair assessment?

WHITCOMB: Yes. There was—and this is something that Barry talks about in his oral history—an NSF review that was crucial to Robbie losing his position.

ASPATURIAN: This was more or less after the Drever thing had died away a bit?

WHITCOMB: After the Drever thing. Ron had been separated, and things were supposed to be getting going, but Robbie was still feeling very frustrated and attacked. The NSF set up a management review of the project.

ASPATURIAN: This was '92, '93?

WHITCOMB: Either late '93 or early '94.

ASPATURIAN: OK. I can confirm the date. [It was early 1994. *–Ed.*]

WHITCOMB: I'm not sure whether what happened with this review was purely an accident, or whether it was actually intentional—NSF's way of getting Robbie out of the way. It was scheduled to be a three- or four-day review: The way it works is that NSF puts together a committee of scientists from various places, and gives them a charge—"review these elements of the project." We're supposed to be given that charge too, so we can prepare the appropriate material to present to the committee. The only thing was, we got a different version of the charge than the NSF committee did. The committee got a charge that asked them to review technical things—for example, how are we doing on writing specifications for this, that, and the other thing—but also our cost and schedule planning. And that last part was left off for us. We didn't actually have that bullet on our charge. The one we were given had no statement about cost and schedule. We thought it was purely a technical review. So the committee shows up, and they want to see all of our cost estimates and our schedules and so on.

ASPATURIAN: So you were a little blindsided by this request.

WHITCOMB: We were completely blindsided. When we actually complained that we had been given a different charge than they had, at least one of the committee members said, "Well, in a real project you should be ready to provide us with cost and schedule information at any time; that's so widely known that it shouldn't have to be mentioned in your charge." And there's probably some degree of correctness there, although I don't think that was the NSF standard at that time. The NSF was only starting to do big projects then, and they didn't really have a standard for how they managed big projects and so on. They were adopting and adapting the DOE [Department of Energy] methods, but we really got blindsided with that. As I say, I don't know if it was intentional because they wanted to have a reason to get rid of Robbie, or if that was just an accident on NSF's part that they gave the committee a different draft of the charge than we got, but I still wonder how it happened.

ASPATURIAN: Why, after Vogt's conspicuous successes in some ways on getting the project moving, funded, organized, and running systematically, did NSF seek to remove him? Was it mostly the mismanagement, or the excessive management, of the Drever affair, or were there other issues involved?

WHITCOMB: I think they were maybe getting a lot of pressure because of the Drever affair. I think they were also finding that Robbie was a difficult person to have to deal with, in the sense that he made strong demands of NSF.

ASPATURIAN: I see.

WHITCOMB: He was asking for more money, and he was pushing back some of their ideas. For example, NSF wanted to set up these review panels to meet every six months, and Robbie didn't want them every six months; he wanted them no more than once a year.

ASPATURIAN: There were control issues involved here.

WHITCOMB: There were some significant control issues, and of course, the NSF gets the right to set those conditions if they're providing the money.

ASPATURIAN: Tom [Thomas A.] Tombrello [Goddard Professor of Physics, d. 2014], as he always did, had some interesting observations on this. I don't know if you've read what he had to say about LIGO in the 1990s LIGO oral history series? [http://oralhistories.library.caltech.edu/182/] This will probably sound familiar. He said, "Robbie runs tribes well, but he doesn't run teams well." And then he talked about his own experience in industry and said certain people will get to some point that other people couldn't have reached, but they can't go farther. And then it usually takes somebody else to carry it to the next stage. And his analysis was that this was basically what happened with Robbie Vogt and LIGO. How do you feel about that?

WHITCOMB: That's probably correct. It's also what happened with Ron. Ron made an important contribution at some stage, but then needed to be boxed out of the project in order to make it happen. Robbie was the right guy to take it the next stage—

ASPATURIAN: So like the sections dropping away from a spaceship as it reaches some point in the trajectory?

WHITCOMB: At some point, yes. So, maybe Robbie needed to be—jettisoned, sadly. Tombrello described Robbie to me once, and I thought his characterization of Robbie was quite telling. He said, "If you have a difficult problem that involves science and technology and budgets and personalities and constraints—a really tough problem—and you take it to Robbie, sit down with him and talk about it and explain it all to him, and then ask what you should do, there is no better person to solve that problem than Robbie. But if you don't have a complex problem and you talk to Robbie, he will create one for you."

ASPATURIAN: It sounds like Tom. [Laughter]

WHITCOMB: There are lots of things I disagree with Tom about, including much of what he claims his role was in LIGO, but in that he actually got Robbie absolutely correct.

ASPATURIAN: He also says in his LIGO oral history that it was his idea to bring Robbie on in the first place.

WHITCOMB: As far as I know, but that I can't comment on. It may well have been.

ASPATURIAN: So this NSF panel came in, and how did things proceed from there?

WHITCOMB: We got a very bad report, failed it.

ASPATURIAN: On every metric? It sounds like the science was proceeding at a fairly successful clip. What did they—?

WHITCOMB: Well, the committee thought they were doing a management review, and we thought they were doing a kind of a scientific progress review, so there was a pretty bad mismatch. I think we failed pretty badly. I can go back and maybe look at the report or something, but we were given pretty bad marks, and NSF was starting to say, "Well, we should pull the money back," and so on. And that was the thing that precipitated Robbie being called to Washington and fired.

ASPATURIAN: He was literally fired by the NSF? Did you or he have any inkling that this was coming?

WHITCOMB: No. And I don't actually know— There's a description of the firing in the other LIGO oral histories, and I think I won't even try to describe it myself. I think I remember him being fired at NSF, but at the same time I think NSF was also saying, "No, this is Caltech's problem to solve, not NSF's; we're not going to fire him. Caltech has to do it." So I don't remember exactly how it all happened.

ASPATURIAN: I seem to recall reading in at least a couple of places that preceding this NSF review, he had evidently had some sort of tantrum in front of an NSF official, and that this created a certain amount of concern as to what was going on with the management of the project.

WHITCOMB: I have read about that incident, but I wasn't there, so I can't really comment.

ASPATURIAN: So when Robbie was dismissed in '94, what was your reaction to this? Did it take you completely by surprise? This was the second such dismissal you'd seen on your watch.

WHITCOMB: I don't know if it took me by surprise. I was probably a little bit surprised but not enormously surprised to see it all happen. Things looked like they were unraveling. When that happened, Charlie [Charles W.] Peck [professor of physics, emeritus, d. 2016] was the [PMA] division chairman. He came to me and asked if I would serve as the acting director while they tried to resolve this, and I agreed to do that. So I was the acting director for a period of time.

ASPATURIAN: Did you have perhaps hopes that you might be made the new director? Were you interested in that at all?

WHITCOMB: No I didn't imagine that they would choose me at that time. They needed somebody more seasoned, and they needed somebody that Caltech had more confidence in than they would have had with me.

ASPATURIAN: Well they had confidence enough to ask you to be the acting director.

WHITCOMB: So I was acting director, holding things together for a period of just a few months, and then they got Barry [Barish]. I think in *his* oral history he says that he had a month to think about it, and so he and I talked at various times during that period of time, and then he came in and started to bring in his own team. He came to me and said, "Look I'm going to choose somebody else as my deputy, but I'd like you to stay with the project and we'll find something for you to do." And you know, I still wanted to see this project through to its conclusion, and just from that discussion I knew that I didn't have what he was looking for in a deputy, and so the best thing for the project was for me to adapt and accept that.

ASPATURIAN: Whom did he hire?

WHITCOMB: He brought in Gary Sanders.

ASPATURIAN: Oh that's right, I know that name.

WHITCOMB: Gary also did a LIGO oral history. He had been working with Barry on the GEM [Gammas, Electrons, and Muons] detector for the SSC and was still at Los Alamos after the SSC went under. When Barry agreed to take this job, he went immediately to Gary Sanders to ask if he would come out here.

ASPATURIAN: I see; he chose somebody he was familiar with and had worked with.

WHITCOMB: That's right.

ASPATURIAN: You mentioned the last time I saw you that you and Robbie had not spoken for, you said, twenty years. Was the falling out over his dismissal?

WHITCOMB: Not *this* dismissal. What happened was that Barry was bringing in his own team of people. Gary brought in Phil Lindquist from the Jefferson Lab [Thomas Jefferson National Accelerator Facility] as the financial manager. Barry and Gary started looking hard at the people that were here and getting rid of people that they either didn't value, or that didn't get along with them, or that didn't accept the new regime, and there were some in each of those camps. But Barry made the decision he wanted to keep Robbie active and engaged in LIGO. I think as much as anything, that may have been trying to find a place for Robbie that would give him some compensation and satisfaction—something in return for having been fired as director.

So Barry brought in a structure that had him and his deputy at the top of the org chart and then under that, at the next tier down, they had four organizations. They put me in as the R&D group leader—that was the smallest of the four groups. I was going to manage the R&D activities at Caltech and MIT. There was a financial and project controls group headed by Phil Lindquist, which would manage the schedules and the budgets and all that kind of thing. The other two groups were the big groups. One was the facilities group, which was going to build the buildings, the facilities, the vacuum systems, all of that kind of stuff.

ASPATURIAN: Infrastructure.

WHITCOMB: Infrastructure. They brought in Mark Coles, another high-energy physicist from the SSC, to manage that. And then Barry wanted Robbie to take over the detector group, which was going to take the results from the R&D activities under me to design and build the actual detector hardware. And the breakdown of the budgets was that 60 percent was to go to the facilities, 20 percent to the detectors, and 10 percent each to the project controls group and the R&D activities. That was the rough breakdown of things. So Robbie went in as the detector group leader.

ASPATURIAN: He accepted the position.

WHITCOMB: After a long courtship. Robbie and Barry would get together once or twice a week for a two- or three-hour afternoon session where Robbie would complain about all the things he would complain about, and Barry would talk about his vision for the project, and I don't know exactly what—it was all hush-hush. I heard from Barry and from Robbie that they talked, but not any detail about what was said. But ultimately, Robbie finally agreed to do this after six or eight weeks of weekly meetings between the two. He started running the detector group, and I was running the R&D group, and so we were working together moderately closely. Robbie moved out of the director's office and into his office down at the end of the hall, which you've possibly been in.

ASPATURIAN: I have never met Robbie Vogt.

WHITCOMB: And I had an office someplace here on the third floor, and we would cooperate. His group was doing designs and design reviews and so on, and I would serve on some of his things, so we were pretty cordial. That went on for a year and a half or two years, something like that. Robbie actually did some pretty good work during that period of time; he got many of the design requirements organized and so on, but I think he chafed quite a bit at having lost the top job. Here he was, going to meetings and having to report to Barry.

ASPATURIAN: He was in a subordinate position.

WHITCOMB: He was in a subordinate position. He's actually older than Barry, more senior, and had had more experience. So it was all the way around unlikely to be a stable situation for the long term and eventually—I don't remember why—Robbie and Barry blew up at each other, and Robbie announced that he was resigning; he couldn't take this any more; he had to step down.

I don't think Robbie really wanted to leave: I think he really wanted to continue, but he thought that his threat to resign as detector group leader would force Barry to cave in on whatever they were disagreeing about. But Barry didn't. And Robbie believed that I should have said, "I will also resign my position unless you cave in on whatever it is that Robbie wants and keep him as detector group leader." So I was supposed to fall on my sword at the same time that Robbie was falling on his. And I didn't. Now we can talk about a little bit about how his situation was different from mine, but I didn't do it, and he viewed that as being disloyal.

ASPATURIAN: Betrayal.

WHITCOMB: Betrayed. And refused to talk with me for some time after that. I think he didn't know exactly what he was asking of me.

ASPATURIAN: He actually came to you and said I'm going to do this; what are you going to do in response? Something like that?

WHITCOMB: No, he didn't ask me what I was doing. He said: "You have to stand with me."

ASPATURIAN: I see; he was more direct.

WHITCOMB: Yes. "You have to stand with me. They can't take on both of us; the project needs us both. And without both of us, it will fail."

ASPATURIAN: I'm surprised. I would have thought that his experience with Ron Drever and with his own dismissal would have taught him that no one is indispensable. But evidently not.

WHITCOMB: Robbie is somewhat blind when he's looking at his own situation. He's much better when he's looking at other people's problems. And his situation was very different from mine. He's in a tenured faculty position, so when he gets dismissed from LIGO he goes off and does something else, and he draws his faculty salary, and life is good.

ASPATURIAN: That is correct.

WHITCOMB: If I threaten to resign and it's accepted, I'm out looking for a job on short notice, and with iffy recommendations from my last supervisor.

ASPATURIAN: You don't have that safety net.

WHITCOMB: I did not have that, and I don't think Robbie ever really realized what he was asking of me. But I think he does believe that I enabled him to be fired, because the first thing that Barry did after this happened was that he came to me and said, "Stan, I want you to take over as detector group leader."

ASPATURIAN: I was about to ask if that's what happened, yes. So you managed both groups.

WHITCOMB: Yes. By that time the R&D activities were kind of winding down anyway, as we were applying everything to getting the detectors working. And so I took over as detector group leader and moved forward with designing the detector.

ASPATURIAN: What was your job title through all this? Were you the senior scientist?

WHITCOMB: [Laughter] Caltech has a funny system. When I was first hired as Robbie's deputy, the official job title for me in the HR [Human Resources] system was "deputy director of the LIGO project." When Barry took over, he brought in Gary Sanders as his real deputy, but nobody actually bothered to change my job title with HR. So, for a number of years, HR continued to carry my job title as "deputy director of the LIGO project." I'd only see it once a year, when I got my annual salary increase notification, and I doubt that anyone else ever saw it. But the job title that really matters at Caltech is Member of the Professional Staff.

ASPATURIAN: I've heard you referred to as the senior scientist also.

WHITCOMB: Chief scientist.

ASPATURIAN: Chief scientist, lead scientist.

WHITCOMB: Yes, within the LIGO organization, many of us had functional titles that the project would make up on its own. And that's one of these functional titles that had nothing to do with my official job title in the HR system.

ASPATURIAN: During these three or four years— I don't know if you're interested in the theater or if you've ever seen or read anything by [the 19th century playwright August] Strindberg, but I mean the clash of emotions and egos, and the dysfunction— how in the world did you keep your equanimity while all this was going on?

WHITCOMB: Who says I did? I would go home and kick the dog every night.

ASPATURIAN: I'm sure you didn't kick the dog.

WHITCOMB: OK, no. Not the dog. No, it was incredibly stressful, and I would go home and rant and complain and—I think you can even see that in my earlier oral history, the one where I said, "Look, I just can't talk about it."

ASPATURIAN: Ah, you were too close to it.

WHITCOMB: It was far too close even at that time. That previous oral history was probably not long after I'd taken over as detector group leader—a year or so after that. We were busy trying to move things forward on a schedule, and things weren't working all that well. It was difficult.

ASPATURIAN: Where was Kip Thorne while all this was going on? Did you have much interaction with him? Was his influence throughout all this for good or bad, or was he just kind of floating above it all?

WHITCOMB: I didn't have much interaction with Kip during that period of time. I think this was about the time he bought his place up in Oregon, and so he would disappear for long periods of time. He tried to help mediate things while Ron and Robbie were having difficulties and when Robbie was having difficulties with the administration here and in NSF, but almost as soon as Barry took over Kip faded into the background. Robbie, when he was LIGO head, used to ask Kip to do a lot of things—talks and that kind of thing—but Barry didn't use Kip as much because he wasn't trying to sell the project so much.

I think Barry felt he knew what the project needed better than Kip did anyway, and he was probably right; and so he didn't particularly engage Kip, and I think Kip used that as an opportunity to kind of fade back and do his own thing. Kip was at that time really trying to seed the relativity community to do much more numerical relativity for the large numerical codes that would eventually calculate the waveforms needed to interpret what LIGO would be detecting. That was all very important, and I think Kip felt that he could do that, and that if Barry had the project moving forward, Kip didn't need to do much.

ASPATURIAN: Do you recall how you felt as the project leadership transferred from Robbie to Barry in terms of your interactions and responsibilities? What did all that mean for you, and what do you think it meant for the project, at least in the early period when that happened?

WHITCOMB: Well for me personally, I was feeling very much that I'd been sort of shoved aside. When I had originally stepped into the deputy director position, Robbie had been pushing me as the scientific face of LIGO in many ways, so I was giving talks at conferences and colloquia here and there. Barry, when he came in, said, "You've been giving too many talks, Stan. I'm going to put you in charge of distributing talks to others. When people come in and want to talk about LIGO, I want you to find somebody to do it, and I don't want you to do so many of them yourself."

ASPATURIAN: What was his rationale for this?

WHITCOMB: It was a good one, which is that the opportunity to give talks should be distributed among the scientists who are working on a project, and there were a lot more scientists then, and they should be doing those things and I shouldn't be doing so many of them. Now, Barry himself was doing a lot more talks.

ASPATURIAN: [Laughter] That was going to be my next question.

WHITCOMB: But Barry was also trying to establish himself as the leader of LIGO. He wanted to put a different face on the project, and so he was giving quite a few talks. So part of it was pull me back, put himself forward. That was fine. But it did pull me back again from engagement with the broader scientific community.

ASPATURIAN: Sure, it was a change for you.

WHITCOMB: There was also—I felt like I had been involved in a lot of the facility design stuff, and with the new way they'd compartmentalized things, I wasn't consulted about a lot of the decisions that were made about the facilities. Never visited any of the sites until they were ready to have detectors installed—well except for one group tour associated with a meeting at LSU.

ASPATURIAN: Even though you played a prominent role in the initial assessment of them.

WHITCOMB: I wasn't invited to the groundbreaking up at Hanford [Washington], even though I was the one who put the contract in place, finished the environmental impact report, and got all that stuff going. So I felt kind of shunted aside. Something, just a minor thing that really tweaked me is that on his online calendar Gary Sanders had what he called the project leadership lunch that occurred every Monday. That was Barry, him, and Phil Lindquist, and I always felt like that was—gosh, it just really tweaked me that I was not a part of the LIGO leadership. OK. It bothers me a lot less now than it used to.

ASPATURIAN: Do you think for Barry Barish there was some sense of, were you Robbie's loyalist and could he rely on you to transition to his leadership on the project? And once he was satisfied with that—

WHITCOMB: I think that's certainly possible, yes.

ASPATURIAN: It's a common reaction.

WHITCOMB: It's a common reaction. I think that was very true of both Barry and Gary Sanders. Both were concerned about where my loyalties lay, and Gary in particular didn't want to have anybody that was disloyal, and he, I think, really in many ways pushed some people out of the project. Bob [Robert] Spero had been with the project forever, and I really think Gary intentionally decided he wanted to move him out. His approach was to challenge Bob to do things that he was not going to succeed at.

ASPATURIAN: That's the classic way to do it.

WHITCOMB: And you just keep doing that until he reads the handwriting on the wall, and he did, eventually. And so over the first year or so of the transition, we lost a lot of people. I survived, partly by doing things like not talking to Shirley Cohen about this back in 1997 [in the first LIGO interview]. Part of the reason I didn't want to talk about this before was that I felt vulnerable and I didn't want to lose my job.

ASPATURIAN: Of course. That makes perfect sense. You and Barry, I guess, gradually developed a good working relationship and confidence in each other.

WHITCOMB: I think we have a decent working relationship. I wouldn't characterize it as a friendship, but it's a cordial relationship. I respect him, and I think he's done really an outstanding job of things. He and I can disagree about things, and it doesn't cause us any particularly long-term harm. Yes, Barry and I get along fine.

ASPATURIAN: It's hard for me to think of a modern scientific project that has engendered so much upheaval and been through so much storm and stress as LIGO until the late 1990s when things finally settled down and you actually did your interview with Shirley. So in closing out this session, where do we find you professionally at about the time you talked to Shirley? What was happening with you?

WHITCOMB: So at that time I was detector group leader. We had let contracts for building the hardware; we were moving forward, training people. We had a new crop of scientists here on campus learning this; we had new electronics engineers that were engaged in this, trying to pull all of this together. I was at that time doing a lot of travel to manage subcontracts—we had contracts for optics, for lasers, for seismic isolation parts.

ASPATURIAN: Did you enjoy doing that, or would you have been happier back doing the hard science?

WHITCOMB: No, I was happy doing that because that was what was going to detect gravitational waves. So in '97, we were starting to get our first hardware parts delivered and all that kind of stuff and beginning to plan the installation of the detectors. The vacuum systems were being installed at the sites, and all that was happening.

ASPATURIAN: Did Barry restore the confidence of the NSF in fairly short order after he came in? How long did that take, do you recall?

WHITCOMB: Almost immediately.

ASPATURIAN: So had you had a sense in the years leading up to this, with all the activity with Robbie, that "My God, this project is going to fall apart again," or were you confident that somehow or other it was going to continue and that it would ultimately lead to something?

WHITCOMB: I was *hopeful* that it would. I don't know that I was *confident* that it would all come together.

ASPATURIAN: By the time you talked to Shirley, though, in '97, you end on the note "I think it's going to work. I don't know when exactly, but it's going to happen and we'll make great discoveries." So morale must have improved greatly by then.

WHITCOMB: Yes. It was much better. I think about the time that I talked to Shirley I had just explored the possibility of going to Germany.

ASPATURIAN: As part of a job offer?

WHITCOMB: Yes. The beginning of the detector group stuff was in '96, and I wasn't sure that was really what I wanted to do. I started to poke around, and I had an offer from Karsten Danzmann of the Max Planck-Institute to go there as a senior scientist. I would have gotten back into doing laboratory work in gravitational waves. And so Laurie and I went to Germany and explored that, and then I made a conscious decision that the prospects were better here than in Germany.

ASPATURIAN: Why did you think that?

WHITCOMB: Because we were actually making progress on the design of the detectors. I could see the sites making progress. It just looked like it was going to come together.

STANLEY WHITCOMB

SESSION 3

May 22, 2017

ASPATURIAN: One thing I wanted to ask you last time and didn't is that Gary Sanders says in his oral history [http://oralhistories.library.caltech.edu/181/] that when he first arrived, he felt all the denizens of the LIGO project were like inhabitants of a battered women's shelter. Had you read that?

WHITCOMB: Yes.

ASPATURIAN: What did you think of that assessment?

WHITCOMB: I think it was a moderately perceptive assessment. I mean, the episode leading up to Robbie's firing, along with the break with Drever and all the other activities on campus, left most of us feeling pretty beaten up and attacked. It was a very difficult time. There were many people on campus who were opposed to LIGO in general and thought of this as a great way of getting back at LIGO for stealing all the resources in Caltech.

ASPATURIAN: So it was people at Caltech who felt that way.

WHITCOMB: Yes, it was people at Caltech. There were a lot of people who really opposed LIGO.

ASPATURIAN: Within the [PMA] division?

WHITCOMB: Within the division, yes. They felt like we'd gotten too many positions and too much lab space and that Caltech was devoting too many resources to this; and it wasn't scientific like the rest of campus, and it was changing the character of campus. You can read that in Peter Goldreich's oral history as well [http://oralhistories.library.caltech.edu/180/]. There were some very, very harsh things. I remember Robbie coming back from one meeting with a senior faculty member to whom Robbie had said, "Don't you want to hear what the LIGO team has to say about Drever and his interactions with *them*, how much he's been disruptive?" And that senior faculty member said, "No, we don't want to talk to them; they're third- class scientists."

ASPATURIAN: Who was this?

WHITCOMB: That was Steve [Steven E.] Koonin, during the time he chaired the Academic Freedom and Tenure Committee. Robbie came back from this thing and told me that directly, and I believe it. It wasn't just "second-class scientists"; it was third-class.

ASPATURIAN: Third-class—a very important distinction.

WHITCOMB: Third-class scientists. I don't know if Steve really felt that way, and I am certain that he did not intend for his comment to get back to us, but it hurt a lot to think that was how the faculty thought about us. Robbie was quite open at sharing these things, and so we would hear them, and I think the LIGO staff got very, very demoralized. We were promised things, and then the promises were broken. At one point, Charlie Peck said, "Oh, I'm going to fix this; we're removing Ron," and then, all of a sudden Ron is back. So there were lots and lots of broken promises, distressing things that were said, and a lot of the people on the project felt like they weren't being appreciated and we weren't making progress, or that they weren't being appreciated for the progress that they *had* made. And so, yes, a battered team is a pretty good assessment, I would say.

ASPATURIAN: What impact did Barry have on morale when he came in? We touched on this last time, but—

WHITCOMB: I think Barry himself will tell you that he's not very touchy-feely.

ASPATURIAN: That comes through a bit in his oral history [http://oralhistories.library.caltech.edu/178/], yes.

WHITCOMB: He doesn't think about those things. He assumes that if you do the best science, then that's the way to inspire people, and if there's a morale problem, then the way to solve it is to do good science. At some level, I agree with and admire that. But I don't think he immediately improved morale; in fact, I think the change in leadership was in some ways detrimental, at least at the beginning. Barry and Gary immediately brought in their team of people and put them over the people who were already here. They clearly favored new ideas over old ideas. I don't think they appreciated, at least in the beginning, the technical difficulties and the technical accomplishments that had been achieved, and so I think people did feel somewhat unappreciated there. Different people within LIGO may have different viewpoints on this. Personally, I always felt a bit more of a kinship with Barry and felt like he understood the situation, contributions, and scientific issues better than Gary did. David Shoemaker, who was the deputy detector group leader while we were designing the detectors, always felt more comfortable with Gary than Barry.

So different people had different interactions. Bob Spero had just a terrible chemistry with Gary Sanders and eventually left. There were a large number of people who had been brought in by Robbie and eventually decided they weren't valued; they just didn't see a place for themselves here and they left. Many of them went to JPL; others I don't know exactly where.

ASPATURIAN: Did Barry's arrival and new stewardship of the project restore confidence at Caltech among some of these institutional critics and faculty whom you mentioned?

WHITCOMB: I don't think it totally reestablished confidence or generated a lot of support for LIGO, but it was clear that the administration was still behind LIGO.

ASPATURIAN: Meaning President Everhart at that time.

WHITCOMB: The president, the provost, and the PMA division chair were all behind LIGO and wanted it to succeed. And Barry was a formidable enough director and presence that it was pretty much impossible for anyone to challenge the scientific credibility of something once Barry said this is what I want to do as my scientific activity.

So it sort of damped down the criticisms. I doubt that most of the critics felt suddenly, "Oh gee, LIGO's a great thing," but they probably felt that they couldn't do anything much about it at that point.

ASPATURIAN: I see. Somewhere in the midst of all this, Caltech gets a new president, David Baltimore [Caltech president, 1997–2006]. Baltimore arrives in '97, I believe, and two years later PMA gets a new division chair, Tom Tombrello. Any thoughts on how that affected LIGO going forward, at least in the short term, or the long term too, if you like.

WHITCOMB: I don't think Baltimore had any significant impact, as far as I can tell, on LIGO or significant opinions about it. He's a smart man, so he must have known how to spell it, but beyond that I don't know. Of course, at that time I was pretty much relegated to mid-level management kind of activities, so if Baltimore got briefed on LIGO or scheduled a visit to one of the sites so he could see what was going on there, I would not have been involved. No one would have consulted with me about that or asked me to participate in it, so maybe he was more involved than I give him credit for, but as far as I know, he wasn't involved much at all. Tombrello—he came to be division chair, when was that?

ASPATURIAN: End of '99, beginning of 2000.

WHITCOMB: OK, so that was actually five or six years after Barry came in and took over. By that time we had already completed most of the major facility construction at the observatories and were installing the detectors. So at least initially, my interactions with Tombrello, or my knowledge of what he was up to or what was going on, would have been very limited.

ASPATURIAN: I see. You were doing something else.

WHITCOMB: From basically '94 to '98, I was overseeing the design and construction of the detector components, the optics, the electronics, the mechanical parts—all of that. In addition, from roughly ’96 to ’98, I was doing a lot of travel to vendors that were producing detector components. And then in 1998 we began the installation.

ASPATURIAN: At the two sites.

WHITCOMB: At the two sites. And I realized at the time we started the installation that we had almost nobody at either site who had any experience with the laboratory-scale work that we had done. Fred [Frederick] Raab had gone up to Hanford, but he was basically the only person there with any significant experience on the 40-meter, and he was busy with all the other things—building up a staff, community relations—

ASPATURIAN: So he was the manager of the Hanford site.

WHITCOMB: He was the Hanford manager, the site head there. He had a big managerial role. And so I negotiated with Laurie, my wife, that I would go and spend some significant amount of time installing and commissioning the detectors at the sites. My agreement with Laurie was that I would spend about half time for six to nine months commuting up to Hanford, overseeing the installation of the detectors and getting them going. It was a little easier for me to do it than it would have been for a lot of the other people. I’m married with no children, so when I go away and come back, you know, a year later, my wife’s three percent older than she was when I went away to do this, whereas if you’ve got young children, they might be 30 percent older than they were when you went away. So there’s a much bigger change.

ASPATURIAN: I understand.

WHITCOMB: So starting in ’98, I started regular commuting up to Hanford. I would go up on a Sunday, stay for nearly two weeks, work twelve straight days, and come home on the evening of the second Friday.

ASPATURIAN: What were you doing up there, exactly?

WHITCOMB: A little bit of everything. We'll come back to that. So I did that, and then I'd spend a week here on campus, catching up with all the paperwork that needed to be done, because there were still fabrication activities and design things to do. I would spend all of my time doing that for a week, and then I would go back up again. So I was two weeks up, one week off, and I did that for four years. And I can remember when I was actually, for a change, in Louisiana, talking to my wife from my hotel room, and she said, "We need to really look again at this agreement that we made." So I negotiated another six months from her. It was basically a four-year period of time, from 1998 to 2002. And during that period of time, I was largely disconnected from things on campus, so again, Tombrello might have had a large impact on things, and I might just not have noticed it at all.

ASPATURIAN: Let's double back and talk about your role in the detector group, heading it up. Based on what you said last time, you thought you had perhaps been leading it for six months when you talked to Shirley, or was it a little earlier?

WHITCOMB: I think it was a little earlier than that but I don't know for sure. I apologize for not actually going back and looking at all these dates. I think Barry took over in '93 or thereabouts.

ASPATURIAN: '94.

WHITCOMB: Beginning of '94. Robbie was the detector group leader for a period of probably a year and a half to two years

ASPATURIAN: Before the great cleavage took place. [S*ee Session Two*]

WHITCOMB: Yes. And then I took over as detector group leader immediately after that. So I think it was probably 1996.

ASPATURIAN: Were you surprised when Barry Barish asked you to take over the detector group?

WHITCOMB: No, I wasn't. We got along reasonably well. I thought there was decent mutual respect on both sides, and so I was not surprised. So during that period of time we had to do the design of the detectors, all of the analysis, the decisions about where do you put the optics; how far apart are the mirrors; what's the length of this cavity and what are the requirements for that length; what are the requirements for the alignment of this mirror, and how do you achieve that; what's the electronic chain that controls each optic so that it's stable, and so that the noise in the electronics doesn't move the mirrors more than is allowed. Those sorts of activities. And most of the choices required balancing a set of requirements in one subsystem, like the suspension system, with another one in the electronics, or a different requirement on the seismic isolation system.

One of the unique things about LIGO is that it's not easy to compartmentalize the requirements for different pieces. They tend to be much more interconnected than I think is usual in most large science projects. We would be continually trying to adjust the requirements because maybe the electronics guys were having a hard time getting the electronics noise below a certain level, and you would try to compensate for that by doing something a little differently over here with the optics. And so we were continually trying to juggle all of those requirements and still keep everything on track for something that you could actually build. If you make the requirements too tight, then when you ask for companies to agree to build this component for a certain price, nobody offers to do it. So there's this balance between producibility and cost and requirements that we were continually juggling during that time.

Dennis Coyne became our chief engineer during that period. He was somebody who Barry brought in, and probably was, I would say, the most crucial person brought into the project in those days. He's probably had the most positive impact of anyone that we had on the project. He's an engineer, so he doesn't get the credit that he deserves on a scientific project, but within LIGO I think that of everyone we had, he was the person who was most irreplaceable. So he oversaw all of the engineering design. I oversaw the scientists who were feeding requirements to the engineers who we were working with.

ASPATURIAN: I see. It sounds like you also did a lot of hands-on and administrative work. If you had to sort of divvy up your portfolio, what percentage of what would you say it was?

WHITCOMB: That's a hard thing to estimate. I would also be involved in preparing bidding packages for contracts and negotiating those contracts, and so on. Probably 50 percent technical, 50 percent administrative—something like that.

ASPATURIAN: How many hours a week were you working?

WHITCOMB: A huge number. I was doing a lot of travel during that period of time, to vendors, to companies, to different things like that. It was probably 70, 80 hours a week. I was working almost all the time.

ASPATURIAN: From what I've been able to read, I gather that the design of the mirrors brought you into the realm of quantum optics. Would you talk about that in kind of qualitative terms? I think it's interesting—the challenges you faced there.

WHITCOMB: It is one of the most interesting aspects of this that I found really fascinating when I got into it. It turns out with the initial detectors we didn't quite reach that quantum realm.

ASPATURIAN: This was with the prototype here or at the two observatories?

WHITCOMB: At the two observatories.

ASPATURIAN: OK. What year are we in when you say that?

WHITCOMB: This is up to 2007. Any time in that region. We never got to the quantum realm in the laboratory. And when I talk about the quantum realm, what it really is—there's a principle in quantum mechanics called the uncertainty principle, which says that for any object, you can know its position arbitrarily well, but you can't simultaneously know its momentum. That the uncertainty you have in the position times the uncertainty you have in the momentum always has to be greater than a certain amount, called *h*-bar—Planck's constant. And that's a principle that is well tested with electrons, with protons, with all subatomic particles, but it also applies to macroscopic objects, including the mirrors in LIGO.

ASPATURIAN: Which were how large?

WHITCOMB: The ones that we had for the Initial LIGO detectors were 10 inches in diameter, 4 inches thick, and weighed about 24 lbs. So, 25 cm diameter, 10 cm thick, 11 kg. But in principle they have that same limitation: If you measure the position of one of the mirrors very accurately, you give it a kick in momentum such that if you were to repeat the position measurement a little time later, the kick you gave to its momentum will make it impossible to know exactly where it will be.

And that, it turns out, is one of the fundamental limits to how sensitive you can make a gravitational-wave detector, because what you are doing is measuring the separation between the two mirrors, which in some sense is the same as measuring their positions. When you do that measurement, you give a kick to those mirrors. So when you come back a little bit later and measure their separation again, which is what you need to do to detect a gravitational wave, they will have moved by an unknown amount due to the kick that you gave them the first time you measured them.

That is one of the fundamental limits, and it comes about because light comes in quanta called photons. There's a finite number of photons in a laser beam—about 10^{20} per second in our laser beams. That's a huge number of photons, but nonetheless, it's a finite number and there are fluctuations in that number given by the square root of n. So if you have 10^{20} photons per second, there will be fluctuations around that of 10^{10} photons. The number of photons determines the kick that the mirrors get. To measure the position of the mirrors as accurately as possible, you want to use as many photons as you can, but the more photons you use, the larger the kick that you give the mirrors.

So there's a balancing act that you do there called the standard quantum limit. And Braginsky was the—

ASPATURIAN: This is Vladimir Braginsky [Moscow State University]?

WHITCOMB: Vladimir Braginsky—he also did a LIGO oral history [http://oralhistories.library.caltech.edu/179/]—was really, I think, the first person to recognize this. He and Kip did a lot of the pioneering groundwork for the standard quantum limit and laid out how it applies to gravitational wave detectors. Now the Initial LIGO detectors, the ones that we were designing in the '90s, building around 2000, and operating until 2007—their sensitivity was about a factor of ten above that limit. So that quantum limit was safely below the sensitivity we were going to achieve with those detectors, but a factor of ten is not a very large factor.

And that was one of the fascinating things, for me at least—to be able to measure so accurately. Nobody had ever come close to measuring something with that standard quantum limit on a macroscopic object at the time. And so when we did that with the Initial LIGO detectors and got within a factor of ten of it, that was closer than anyone else had ever done before, at least for such large objects. The Advanced LIGO detectors are actually predicted to reach that limit and in fact beat it a little bit, by a trick that we have called squeezing.

ASPATURIAN: Is that light squeezing?

WHITCOMB: It's light squeezing. You squeeze the uncertainty in the light—its phase and amplitude uncertainty—in a certain way that I can't explain to you without waving my hands and drawing a little picture, but it is possible to do. Because the limit is on the product of the uncertainties in position and the momentum, there are ways of manipulating the size of one of them versus the size of the other to improve the sensitivity in different realms.

ASPATURIAN: It's a type of compression.

WHITCOMB: Exactly.

ASPATURIAN: How did you go about dealing with this in terms of your work? Was a lot of your time taken up with scientific deliberations? Did you spend a lot of time working with the engineers?

WHITCOMB: A lot of time working with engineers, a lot of time in meetings. The meetings were usually working meetings and somewhat productive, but there were a lot of them. I stopped going into the laboratory shortly after I took over as detector group leader, and I didn't have the time to keep up with the laboratory things. I would talk sometimes with the scientists who *were* working in the laboratory, but I stopped going to the laboratory and stopped doing much of anything that way. I spent all of my time reviewing documents and drawings and dealing with design reviews and contract negotiations. Lots of telephone calls, lots of meetings.

ASPATURIAN: Whom did you work with chiefly in these years? This engineer you mentioned—

WHITCOMB: Dennis Coyne, David Shoemaker—

ASPATURIAN: Your deputy?

WHITCOMB: Yes. [*SW subsequently added:* David had about as much experience in the field as I did. He had started at MIT with Rai, then eventually ended up in Europe—first spending a year or two with the gravitational wave group in Munich, where he did the first, or maybe one of the first, detailed experimental noise studies of a real interferometer. He then moved to Paris, where he did his PhD before going back to MIT. He was the natural choice for a deputy, partially because it spread the detector leadership between Caltech and MIT, but mainly because of the breadth of his knowledge and the systematic way he did things. He was a really good deputy and I enjoyed working with him immensely.]

Mike [Michael] Zucker was also a key person during that time.

ASPATURIAN: He'd been a graduate student here.

WHITCOMB: Right. He stayed as a postdoc, was converted to a staff position, and then eventually moved to MIT when his wife got a job on the East Coast. But he was a key player. [*SW subsequently added:* Mike had done some groundbreaking work in the 40-meter at Caltech, unraveling some noise sources that we had not appreciated before. After we got funding for the initial LIGO detectors, he got involved with the vacuum system, and became one of the main contacts between the project and our vacuum system contractors. That honed his skills as a project leader, and we turned to him several times in LIGO when we had an effort that needed forceful coordination between outside suppliers and different folks within the LIGO Lab. He eventually became the site head at Livingston, which he did for, maybe five years.]

ASPATURIAN: Anyone else you'd like to mention?

WHITCOMB: Peter Fritschel, from MIT. Nergis Mavalvala, who had been a graduate student at MIT, and then came out here as a postdoc. She was also converted to a staff position for a while before going back to MIT as a faculty member. Gosh, there's a whole team of people that we were working with. Rolf Bork, our electronics software engineer. Jay Heefner, Rich Abbott—two more electronics engineers. Bill Kells, another scientist who did a lot of the optical engineering and optical design; GariLynn Billingsley, an optical engineer; Hiro Yamamoto, who also did some good optical simulations. A lot of these people are still with LIGO, and some of them have moved to the sites and taken positions there.

ASPATURIAN: LIGO, I think, was notable—correct me if I'm wrong—for having so few faculty actively involved. Much more like a JPL or a Lawrence Livermore, with a large number of scientific and technical staff. Was that a deliberate decision?

WHITCOMB: It was a deliberate decision, originally made by Robbie, but continued by Barry. When Robbie was in charge, he made the decision to have a lot of research staff positions for the project. There are some significant advantages. If you know what you want to do on a project, and you hire research staff, they don't have to worry so much about publications, because they're not being reviewed for tenure. Tenure-track faculty have to publish, because they need to be reviewed and renewed by the faculty. Research staff, once you hire them, they're yours forever, and they can work on stuff, without having to worry about periodic reviews by the faculty. So in the sense that what we really needed were a lot of people to flesh out the design of the interferometer—work out the details, say, of what reflectivity to choose for these mirrors or how to design a control system. Most of that is not very interesting as a publication, except in the context of a gravitational-wave detector.

So it would have been very difficult for much of anyone to have gotten a tenure–track academic faculty job out of this.

ASPATURIAN: This had already happened with you; you foresaw this. [S*ee Session One*]

WHITCOMB: Right. And also Fred Raab. Tombrello talks about this in his LIGO oral history [http://oralhistories.library.caltech.edu/182/]. So Robbie made the decision, similar to one he had made earlier when he headed the cosmic-ray group [in the 1970s], to hire research staff people. He treated the research staff with some degree of respect and treated them more or less as real scientists, so most of us didn't actually recognize that we were these "third–class citizens" on campus for most of that time—I didn't—until the whole thing blew up with Drever and Vogt. So it is a bit unusual in the sense that there were not a lot of faculty, but the faculty, if we'd had more, might not have found it a very rewarding career path. When Tombrello became the division chair, he was very pleased that he recruited Tom [Thomas] Prince [Bowen Professor of Physics] and Ken [Kenneth] Libbrecht [professor of physics] to shift their research and work on LIGO.

ASPATURIAN: I did not know that.

WHITCOMB: So Ken Libbrecht had gotten a little tired of doing solar astronomy at that point, and Tom convinced him to come and worked on LIGO. Tom Prince had been very successful with his gamma-ray astronomy, but his scientific interests had ties to LIGO, and so Tombrello recruited both of them. Tom Prince worked on it for about a year and then decided, "Wait a minute; the stuff I'm doing is not really central to LIGO." I think he didn't think he was appreciated by Barry very much, and his work wasn't very central, at least at that time. All these LIGO people were more concerned about how do you test the surface figure of optics than they were about the niceties of waveforms for black holes. So he kind of got bored and moved over to LISA [Laser-Interferometer Space Antenna].

ASPATURIAN: Which is a space-based LIGO.

WHITCOMB: A space-based detector. There's no detector around, so people just talk about it—what it could do if they had data—and you can publish a lot more papers, and more interesting papers, at least for an academic environment. Ken Libbrecht discovered something that he wanted to study in the laboratory, and LIGO was a convenient place to get money without having to write proposals and so he sort of created his own little corner of this.

ASPATURIAN: Snowflakes? Or was this before snowflakes?

WHITCOMB: No, different from snowflakes. But it was fundamental research and not done with a real aim toward producing information that LIGO would use now in the short run. It was just kind of free-floating, and eventually in one of the downturns when we didn't have enough money, it kind of got shut down. So the faculty members Tombrello recruited into this, thinking, if we just had more faculty in LIGO, then we won't have all those problems we had before—I don't think that really worked out all that well.

ASPATURIAN: The grafting didn't work too well.

WHITCOMB: The grafting didn't work. Alan Weinstein [professor of physics], who did come over, did stick and made a significant contribution in terms of the data analysis somewhat later.

ASPATURIAN: We can get to him. [S*ee Session Five*] He's a particle physicist, high-energy.

WHITCOMB: Yes.

ASPATURIAN: During this period between the late ’90s and the early 2000s when you were heading this group working on the detectors, did you run into any major obstacles that threw you—that you had to deal with, either scientific or logistic or administrative?

WHITCOMB: There were all kinds of things that happened. Fairly early on in the design process for the detectors, in the ’96 kind of timeframe, we realized that the NSF proposal had severely underestimated the requirement for how good the mirrors had to be. The actual construction proposal said, “Gee, what we need are mirrors that are smooth to a level “lambda by four”—one quarter of the wavelength of light, but the proposal actually said we’ll specify them as lambda by twenty because that’s not very hard to achieve. Well it turns out that the real requirement was more like lambda by a thousand—that is, the wavelength of light divided by a thousand.

ASPATURIAN: That’s quite a distinction.

WHITCOMB: It was quite a bit tighter than what anyone had expected, and in fact when we first started asking questions of the optics industry, people said nobody’s ever made a mirror like that. So Rai Weiss went away and found some data and indeed, when they were analyzed in a proper way, it turned out that people *had* been able to make mirrors that were that quality. They just hadn’t been able to really measure them or really, hadn’t been able to understand what they had measured—the limitations were within their measurement. So in some sense what we really had to do was educate the optics industry about what they already *could* do. Not teach them to do something that they hadn’t ever done before, but teach them to measure what they were doing already, and convince them to sign on to specifications based on what they had done in the past. So that was a fairly tricky piece of this and was, I think, one of the biggest challenges, technically, that we had. The optics that we finally got were, as I said, a good factor of a hundred better than what we thought we needed when the proposal was written. I believe that if anyone had really known what the requirement was when we wrote the proposal, it would have been rejected immediately because everyone would have said nobody can make a mirror that good. So in a sense we were lucky not knowing what was required.

ASPATURIAN: That's an interesting point.

WHITCOMB: If we had really understood what it was when the proposal had been written, I think this project never would have been funded. So, perhaps the optics were the most significant technical challenge.

One of the most amusing challenges—we had some rubber parts that go into the seismic isolation. It's just a seat that a spring sits in for our vibration isolation system for the Initial LIGO detectors.

ASPATURIAN: It's like a chamber that insulates the—?

WHITCOMB: No it actually goes inside the vacuum system. There are some springs and masses, and the springs have a rubber seat that they fit into that holds them in place and provides some damping. And because the rubber is inside the vacuum system, it needed to be treated with a special process. It's not a terribly sophisticated process—there's some washing with d-i [deionized] water and a little bit of chemical stuff and some baking at an elevated temperature.

We found a company in Oklahoma that would do it at a good price, and so we had taken something like a third of those seats, and sent them out to this company. They were processing them for us when a tornado came through, ripped the roof off their facility, and scattered our parts all over the state of Oklahoma. And so we had to go back and remake the parts, which delayed our schedule a little. Something I am a little proud of: At that time, it was a small company—a mom and pop shop basically. And so after the tornado, we actually gave them some start-up money to rebuild their facility. They needed some ovens to do this and a d-i water system, so we provided them with a little bit of extra money to get them restarted on this, since it wasn't their fault that a tornado had gone through. We actually got them up and working and got their employees back to work faster than it would have been if they had to wait for new jobs to come in. So we actually tried to be good citizens in that regard.

ASPATURIAN: I'm struck, listening to you, by the level of scientific and administrative creativity that went into this project. There was not a lot of precedent for what you were doing in many dimensions, would that be correct?

WHITCOMB: I think that's right. We did some things in terms of the contracting that I think were somewhat innovative. The beam tubes—these long stainless-steel tubes that the laser beam travels in—are the largest part of our vacuum system. We had budgeted a rather low amount for this—we knew we really needed those to come in at an inexpensive level. Normally the way you would do it would be to have companies bid against each other against a specification, but the specification for this was different enough from the experience of the standard vacuum companies that we didn't believe we would be able to get a good price if we just said, "Give us a bid and then do it." So what we did was we had a two-phase contract where we paid a company on a time-and-material basis to do a small-scale demonstrator and prove to us that they had the techniques, the designs, and the procedures to achieve the kind of vacuum that we needed.

ASPATURIAN: You asked them to build a viable prototype, yes.

WHITCOMB: A prototype. And we paid them on what's called the time and material contract, not a fixed price contract, so whatever they spent in terms of material, labor, and so on, we reimbursed them for, plus a small amount for profit for the company. But for us, the equally important part of it was to have the company learn that they could do this at a relatively low cost. So at the end of that prototype phase, we had an option to go with them for the full-scale device, provided they gave us a fixed price that was within our budget.

It was about a forty million dollar contract—it was a pretty significant one—but we didn't tell them what our budget was. We said, "The contract is yours if you want it, and if you give us a good enough price. And so they weren't bidding against another manufacturer; they were bidding against an estimate of what we thought it should cost, and as a result we got the vacuum system at, I think, a very good price, and it was a very successful thing. But part of the process was teaching them that they could do this at a price that we could afford. So I think we were all-around innovative in terms of the technical things we did, but also with the contracting and how we worked with industry.

ASPATURIAN: There's this flavor of a great deal being made up as you went along, because it was so new and so different.

WHITCOMB: Yes. You know, we were oftentimes pulling different pieces of an existing model from here or there, but I think we *were* pretty creative. Other people can quibble, but I think we did a pretty good job of this.

ASPATURIAN: Looking back at this period, how would you assess the relative contributions of Caltech and MIT to the project? It was this bicoastal collaboration.

WHITCOMB: Again, when Robbie was running the project, he basically determined that the scientific activities be divided between Caltech and MIT, but that the engineering activities all be concentrated at Caltech.

ASPATURIAN: So he could keep an eye on them, basically.

WHITCOMB: So he could keep an eye on them, and also to facilitate the interactions among the engineers. Also the engineers worked closely with the contracts people here at Caltech for bidding on these rather big jobs and lots of little jobs, and it was an advantage to have them near the contracting people. The contract with NSF was set up as a cooperative agreement. Caltech got the money, wrote a subcontract with MIT for the activities there, and so the bulk of the money was kept here at Caltech and shipped out through this contracting activity.

ASPATURIAN: Under Robbie?

WHITCOMB: Yes, under Robbie and even under Barry for a very long time. Throughout this whole period of time, the central administration at MIT was willing to let Rai do this as an individual faculty member and to have as many employees as he could afford from LIGO, but it wanted really nothing else to do with the project.

ASPATURIAN: Why is that, in your opinion?

WHITCOMB: I think they didn't believe it was going to succeed. There is a famous story about Robbie and Rich Isaacson [NSF program officer for LIGO] going to MIT and asking the MIT provost John Deutch, What would MIT contribute to LIGO; what did they want to put into this project? And the answer was, "Nothing; we don't want to put anything in." And that kind of shook Rich Isaacson, and he said, "No, no, we're not asking you to put cash in; just what is it that you want to contribute to this whole activity? Space, or faculty, or whatever. We're just— What is—?" And Deutch took a piece of paper, wrote a big zero on it, held it up for Robbie and Rich to see, and said, "I think I told you clearly this is how much we want to put into this." [Laughter]

ASPATURIAN: And yet Rai Weiss and his people just kept right on working in this climate.

WHITCOMB: Rai, I think, always felt completely neglected and unappreciated at MIT. They did appreciate what he was doing with the cosmic microwave background work. They really liked that, and they were willing to tolerate him working on this crazy gravity thing as long as he kept his cosmic microwave background stuff going.

ASPATURIAN: What do you think it was in the respective cultures of Caltech and MIT that made one so receptive to this visionary prospect and the other very negative or at least indifferent?

WHITCOMB: You know, it's tempting to try to figure out what it is in the culture of these places that make them so different. But I'm going to offer a different perspective, which is that typically these decisions are made by small number of people, and that the crucial factor is the exact combination of people that are at one place or another at a given time, not the intrinsic culture of the places. When LIGO was first embraced by Caltech, we had Kip Thorne here, who was very enthusiastic about it, and an administration at MIT that was not. I think that if we were to redo the experiment now—trying to convince Caltech to do it without having Kip Thorne on the active faculty and trying to convince our current provost [Edward M. Stolper, Leonhard Professor of Geology; Caltech provost 2007–17] to do something like this versus going to the current MIT administration—we probably would find almost the opposite situation. MIT would be supportive and Caltech would resist it. So I think it's not so much the culture of the two places as the randomness of who is in charge at a given time.

ASPATURIAN: Sort of serendipity or contingency. I have a timeline here cribbed from the LIGO website. I see that in 2006, you achieved design sensitivity. What exactly did that mean?

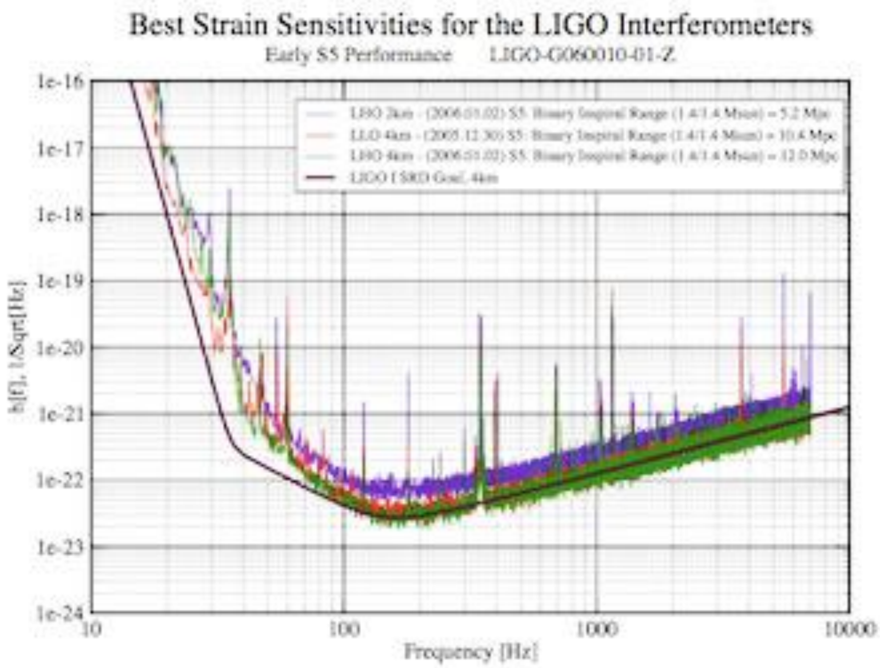
WHITCOMB: In our 1989 proposal, we had a curve that said, This is the sensitivity as a function of frequency that the Initial LIGO detectors will have. And then there was another curve for Advanced LIGO.

ASPATURIAN: So that was the projection.

WHITCOMB: So that was the projection. When Barry came in, that curve got incorporated into a science requirements document, and we reduced that curve to a simpler form, namely the distance that we could detect to a particular type of source—a binary neutron star inspiral and merger—and that became our benchmark. So it was really in 2005, not 2006, that we achieved that sensitivity. So your timeline is off a little bit, but—

ASPATURIAN: Well, it's from your own website. [Laughter]

WHITCOMB: Just because it's on our website doesn't mean it's correct. [Laughter] So in 2005 we achieved design sensitivity, which is a remarkable thing because the original sensitivity curve was based upon a design with a laser that we didn't use in the Initial LIGO detector [argon ion lasers], with a seismic isolation system that hadn't been designed *at all*, intended for a site that we hadn't chosen, so we didn't know what the ground noise was, with a model for thermal noise—the other piece that's important for determining the exact sensitivity—that was *completely wrong*. All of those incorrect pieces had gone into the original curve, and so when we actually achieved it, it was really an amazing thing to achieve. [Bringing out a diagram.] So this is the curve that was in that 1989 proposal.



ASPATURIAN: The red line I'm looking at.

WHITCOMB: The smooth maroon one. And the jagged green line was what we got at Hanford, and the second red line—the jagged red line, not the smooth maroon one—is what we got in Livingston, and you see that they actually match very, very well.

ASPATURIAN: They mirror each other pretty closely, considering that some of your initial conditions were so far off the mark.

WHITCOMB: Considering that so much of the basis for this original design got thrown out completely. So it's an amazing tribute to people just saying, "Here's what we have to do, and we'll figure out a way to do it."

ASPATURIAN: "We'll make it work."

WHITCOMB: Yes.

ASPATURIAN: What were the major milestones between, let's say, the late '90s and your proposal? I guess in 2004, at least according to the timeline, NSF accepted the Advanced LIGO project?

WHITCOMB: Yes. So the Advanced LIGO project was really conditional on achieving what we set out to do with Initial LIGO. And NSF may have accepted the proposal in 2004, but I don't think they actually allocated funding until we had achieved that full design sensitivity in 2005.

ASPATURIAN: There was never any expectation really that Initial LIGO would detect anything, or were there people who thought that it might?

WHITCOMB: I thought it might.

ASPATURIAN: *You* did.

WHITCOMB: Yes. It was by no means certain that it would. First of all, what we could say pretty accurately is that for Initial LIGO we can build a detector that's this sensitive, OK? It's going to be difficult, but this is what we can get with the detector. That part you could say pretty easily.

What you didn't know was, what are the sources? If you can build a detector that sees out to ten megaparsecs, the question is how many of these gravitational-wave events are there in that volume per year. People were guessing that number might be anywhere from one every hundred years to ten per year—that kind of range. And that was the uncertainty in one particular source, the one that we understood *best.*

ASPATURIAN: That was the neutron star model?

WHITCOMB: Binary neutron stars, yes. And so a range of 1,000 in the rate of these events—that was the uncertainty. Not in the detectors. We could build the detectors to see out to ten megaparsecs. The question was—

ASPATURIAN: Would the sources cooperate.

WHITCOMB: —what was Nature going to give us? A lot of people had very pessimistic views of these. We still haven't seen any of the neutron star sources yet. [Two months, after the completion of these interviews, on August 17, 2017, LIGO made its first detection of colliding neutron stars. *–Ed.*] The probability of seeing the kind of source that we did see—a binary black hole system—was even more uncertain than the neutron star sources by probably at least another one to two orders of magnitude. It became kind of fashionable about the time we started to take data with the Initial LIGO detectors in 2005 to say, "We know these detectors aren't good enough to see anything, but they're important for us to actually achieve some credibility and learn how to do the observations, and demonstrate we know how to do it."

I still thought there was a chance we might see something. Now that we know how many binary black holes there are in the universe, or at least have some decent idea based on having detected a couple in that first run, we can now actually go back and say, What was the probability that we might have detected a pair of black holes in that first data run with Initial LIGO? The probability is not huge, but it's not small either. I think it's on the order of 10 to 20 percent chance that there would be a binary black hole merger close enough that we would be able to see it with Initial LIGO—not huge, but higher than most things that have been written would imply. So I think we went through this period of time saying, "Oh, Initial LIGO detectors aren't interesting at all; they're just a demonstration." But in fact, they were not so very far from being able to see something.

ASPATURIAN: So it could have happened.

WHITCOMB: It *could* have happened, it just didn't.

ASPATURIAN: I think next time we'll go into Advanced LIGO and what happened in that era. Is there anything else you'd like to say for this session? I know there was also this international scientific collaboration taking shape. Were you at all involved with that at that point?

WHITCOMB: I was. I think that really more comes in later. I actually would like to just add a personal note, OK?

ASPATURIAN: Yes, absolutely.

WHITCOMB: That in some ways, the most fun time I had in LIGO actually were the four years when I was commuting up to Hanford and working on the detectors, installing them, commissioning them, trying to make them work, trying to make them be more sensitive. It was a grueling schedule: I would go up on a Sunday afternoon. I would work twelve straight days—sometimes twelve, fourteen hours a day—go home to the apartment that we had up there, cook a little bit of dinner, crash, wake up the next morning and off early to work. So it was grueling, but it was also tremendously exhilarating. Up to that time, during that design process, everything looks about this big [gesturing with hands; *SW subsequently commented:* "about 8 inches apart"], because they are just drawings and the drawings are all sized to fit on standard sizes of paper. So if you've got the tiniest little clip that holds a wire down, it's that same size again, and if you have a vacuum tank that's going to hold the whole seismic isolation system, it's also that big [still gesturing with hands about 8 inches apart]. So everything was that big during that design process.

And, then, when I started commuting to Hanford and started to see the actual hardware—the vacuum tanks—coming in, suddenly the vacuum tanks are huge and these clips are really tiny. And there are *little screws*, and you had to— And everything just took shape in a way that was really magical. I was working with a really fantastic team of people up there. Most of them had no experience with laser interferometers, but they were eager to learn.

Taking a break during installation of the optics assembly at the LIGO Hanford Observatory in 2000.

ASPATURIAN: Where were they recruited from? Were they local?

WHITCOMB: A lot of them were. Fred Raab had recruited people from the local colleges, and people who'd worked on the facilities during their construction. We had a guy, Hugh Radkins, who had been a surveyor and laid out the facilities early on, and Fred thought, gosh, this is a guy who's got some good skills and he's a good worker, and we hired him. We had some people fresh out of college--Corey Gray, Betsy Weaver. We had just a variety of people that were hired from the local area. They were hard-working, eager to learn about this, and there was a great camaraderie and a great team of people to work with, and I thought it was just *magical*. I could go up there, spend two weeks lost in the work. Nergis Mavalvala was also spending a lot of time at Hanford during those days, and we got along beautifully, so that added to the joy. Then I'd come back to campus, firefight for a little while to get paperwork done and so on, and then I looked forward to going back up and seeing this whole thing getting built up and making it work.

ASPATURIAN: Watching the vision come to life.

WHITCOMB: Watching the whole vision come to life. It was particularly nice after the stress of Drever, the Vogt era, those things. It was fantastic actually. It had a second effect, which was that a lot of my disconnectedness from Caltech dates from that time, because I was gone all the time, and then the weeks that I was back here I was busy doing all of my other things. I stopped going to seminars; I stopped going to the Athenaeum—I let my Athenaeum membership lapse. After four months of seeing nothing but your membership fee on your Athenaeum bill, you let that go. And so I stopped really participating in campus activities and discovered that in fact, I didn't miss the campus connection at all. [Laughter]

ASPATURIAN: Who was your opposite number in Louisiana?

WHITCOMB: There are two people who did that same kind of job in Louisiana. One was Peter Saulson. Peter had been a postdoc first with Rai Weiss at MIT. Then he stayed on as a staff scientist for a while. He had a bad reaction to Robbie taking over as director. Eventually he left MIT and established his own research group at Syracuse University. He kept working on LIGO-related activities at Syracuse, but outside of the LIGO laboratory, outside of the LIGO project. When we started the installation down in Livingston, it turns out Peter was up for a sabbatical and so we effectively paid him half-salary to extend his sabbatical to a full year and do it at Livingston. So he was stationed there for about a year trying to do the same kind of thing that I was doing up at Hanford. By this time, Peter had grown into one of the most respected members of the LSC, and not too long afterward became the first elected LSC spokesperson, taking over from Rai, who had been appointed into that role initially.

When Peter left Livingston, Rai Weiss sort of picked up that role, again travelling down to Louisiana. So Peter Saulson got a lot of the installation going; Dennis Coyne did a lot of the commuting to Louisiana for installation work; and Rai Weiss was the person who came in and brought the scientific know-how of how do you make this operate, how do you track down the noise kind of things.

ASPATURIAN: Did the intrinsic differences—that I imagine exist between Hanford, Washington, and Livingston, Louisiana—have any impact on how the project evolved?

WHITCOMB: They certainly did, yes. There were real differences between how things had to evolve in the two locations, simply because of the different environments.

ASPATURIAN: You mean physical environments?

WHITCOMB: The physical environments, especially the level of ground vibration. But there were also, you know, different personalities and attitudes of "we have to do it a little differently than they do it at that other site, simply because we have to do something different. We can't be seen as following them."

ASPATURIAN: Oh, in that regard. I wondered if the cultural flavor and the local outlook affected things much.

WHITCOMB: Some, yes.

ASPATURIAN: But you're speaking more of a kind of political jockeying between the two.

WHITCOMB: Yes, and there were certainly cultural differences in the staff as well. The two site heads, Fred Raab and Mark Coles, had slightly different philosophies in terms of the people they hired at the two locations. Partly that's driven by the difference in the labor pools in the two locations.

ASPATURIAN: Anything else today?

WHITCOMB: I think that's it.

STANLEY WHITCOMB

SESSION 4

June 5, 2017

ASPATURIAN: I'd like to begin by noting for the record that as of last week, a third LIGO detection was announced. Would you like to say a few words about it?

WHITCOMB: This one is exciting, actually, for a couple of reasons. It's a little different than the first two in the sense that there was evidence in this one that the spin of the black hole was not aligned with the orbital angular momentum. So that is an important kind of new feature of this.

ASPATURIAN: This is the spin of the composite black hole?

WHITCOMB: No, of the individual black holes. The two black holes orbiting each other have an orbital angular momentum that points in a certain direction. Each of the individual black holes will also have something called spin, which is an internal angular momentum, and that can be either aligned with the orbital angular momentum or against it or just any old angle, OK? And those different cases—aligned with, aligned against, at some other angle—affect the waveforms as the two black holes spiral together. And so we're able to see that in this particular case the orbital angular momentum is not aligned with the spins of the black holes. So that suggests something about the origin of the black holes—that maybe these weren't formed directly from stars that collapsed in a single system but perhaps were put together by a collision between three bodies that left them in an orbit.

So we're starting to learn things, and that, I think, is really starting to show the power of the gravitational waves. With the first events, while it was great to see the gravitational waves, it's also the case that you can come back and say, "Did we learn anything unexpected?" Here we're now starting to learn more detailed science that we hadn't really known about before. It'll tell us a little bit about the origins of these systems when we actually start to see them. The other thing is that these two black holes were about the same masses, just a tiny bit smaller than the first pair that we saw. The second one was quite a bit smaller.

ASPATURIAN: That's right.

WHITCOMB: But, again, this was one of the larger ones, which maybe is also starting to tell us that these large black holes might be more common than we had anticipated. I think many people thought that first one might be a little bit of a fluke—that occasionally big ones like that are seen, but that maybe when we actually got a full population study we wouldn't find that they're so common. The fact that we saw this third one now with such a large mass maybe suggests that we're starting to learn something about the broader population there. So it's all very early days still, but that's starting to really show, I think, the promise of gravitational waves as a tool for astronomy.

ASPATURIAN: And this one was older, I believe. Three billion years to roughly one billion for the first two?

WHITCOMB: Well. [Laughter] Further away.

ASPATURIAN: Yes, farther away.

WHITCOMB: It was roughly twice as far away as the first one, and so the radiation took longer to get to us. And so it's a little bit weaker for that reason.

ASPATURIAN: Does LIGO operate within a certain frequency band? Do gravitational waves have specific frequencies, and are there some collisions of this nature you will not be able to observe for that reason?

WHITCOMB: There are. LIGO does operate in a certain frequency band, from roughly 10 Hz to 5 kHz, something in that range. And that's a region where the sources of gravitational waves will be about the mass of our sun, up to maybe 30 times the mass of our sun.

Larger black holes—the kind, for example, that populate the centers of galaxies, and have masses of maybe a million to a billion times the mass of our sun—are too large to radiate gravitational waves at the higher LIGO frequencies. In order to see those, you'd need to build a space detector, and that's where LISA [*see Session Three*] comes in. It is a somewhat LIGO-like detector in space, but because it can be much longer in terms of its arms, it can see lower frequencies that come from higher mass objects. It's just like electromagnetic radiation, where there are short wavelengths—like light—and long wavelengths—like radio waves—and the long wavelengths tend to come from things that are larger than the small ones.

ASPATURIAN: I understand. I also wanted to mention I found your talk at the International Center for Theoretical Sciences [Bengaluru, India] on YouTube, and thought it was very interesting and informative. I'm mentioning it here because I'm going to put it into the oral history record suggesting that it be kind of viewed as a companion piece to the oral history. ["Whispers from Space: The Detection of Gravitational Waves from a Binary Black Hole Merger"; https://www.youtube.com/watch?v=A4UGrTvS9t8] And you provided some nice technical background that we're not going into here.

So let's see, where do I want to start? Actually I'd like to start with something you said in that talk, when you told the anecdote about [Subrahmanyan] Chandrasekhar, who said to you in effect, "What a pity for such a talented young scientist to waste his time on this project." I wondered if at any point in your career, after you came back to Caltech, you found yourself kind of echoing those words, or whether anyone else said to you, "Why are you still doing this when you could be doing so many other things?"

WHITCOMB: [Laughter] There certainly were times. You know, in my first set of interviews with Shirley, I talked about how I almost went back to infrared astronomy.

ASPATURIAN: Yes, and you went into private industry too.

WHITCOMB: So there have been times when I've gotten discouraged by this whole business and thought maybe a different course of life would have worked out differently. But I—for the most part I don't regret this.

ASPATURIAN: Certainly not now.

WHITCOMB: [Laughter] About two years ago, just after we'd made the big discovery but hadn't made the announcement, I was out to dinner with a few of my colleagues, and Albert Lazzarini, the deputy director of LIGO, asked me how long did I think it would take to detect gravitational waves when I first started with LIGO? So that was in 1980, when I started. I told him that I had actually planned that part of my tenure case would be the detection of gravitational waves, so that's what I was really anticipating: It should be done in six or seven years; that's a long time to work on something, and it should produce results by then. And Albert asked me if I had known at that time that it was going to take thirty-five years to detect gravitational waves, would I have still done this? And I couldn't answer his question; I couldn't actually. I would like to think that the answer would be, yes, I would have chosen to work on this even if I had known that it was going to take thirty-five years to pursue the difficult task, but I'm not sure I would have.

ASPATURIAN: I also wanted to follow up on a couple of things you mentioned in the last interview. You talked about the rather different approaches to the interferometers in Louisiana and Washington, and you said there was a certain amount of competition between the two sites. Did this function as a hindrance or as a spur to innovation as the project went on?

WHITCOMB: I think overall maybe it was a spur to innovation, but it was a hindrance to progress. There's something very satisfying about doing everything yourself, OK?

ASPATURIAN: Yes, I know.

WHITCOMB: There are a lot of people who just—the thing they really want to do is go out into the woods, build their own cabin, work the land, grow their own food, cook it themselves, and do everything themselves. And there is something very satisfying to that. But if you really want to make progress, you need to take advantage of the accomplishments of other people, and if somebody else has figured out how to do something, even if it's not quite as good as you would do it, sometimes you make faster progress taking advantage of what they've done, rather than starting from scratch and trying to redo it yourself. I think when we had the two observatories in the early days, there weren't that many people who really knew exactly how to do everything, and the fact that the two sites tended to not want to learn from each other slowed things down. It did take us longer than I think it should have to go from installing the detector to really good sensitivity. I think we could have done it faster if the observatory staff had cooperated more.

ASPATURIAN: Who was responsible for seeing that they did more or less did cooperate?

WHITCOMB: It was to a large extent *my* responsibility, because I had a role in bringing the installations together and making the detectors work, up until the time when I stepped back from that at the end of the installation and the beginning of the first science run. So that was partly my responsibility—and I should take a large part of the blame for any failures.

ASPATURIAN: You had a lot of other things you were doing at the same time.

WHITCOMB: There were lots of other things. I think some measure of the responsibility for that also should have gone to the two observatory heads, Fred Raab and Mark Coles—to insist that if their staff decided not to do or use something that had been done at the other observatory, they should provide a really good reason for not doing it, rather than just saying, "I don't want to do it that way." And I think the observatory cultures didn't reflect that very well, and the two site heads weren't as successful at fostering cooperation between the sites as they might have wished.

ASPATURIAN: Are there any anecdotes about your time at either of these observatories? Interesting, amusing, illuminating that you want to put into the record?

WHITCOMB: There's nothing that immediately comes to mind. As I said last time [*Session Three*], it was a time that was quite likely the high point to my career with LIGO. I was having the most fun; I was working with great, enthusiastic people; we were accomplishing a lot in terms of putting things together and making them work, and I felt welcomed and appreciated at the observatories. I didn't have to spend much time on campus, where I didn't feel welcomed and appreciated, so it was a time that I enjoyed a lot. But I'm not sure I have any great stories.

ASPATURIAN: You were basically the point person on the detectors, which was the key component, if you will, of this entire assemblage, and you didn't feel welcomed or appreciated on campus?

WHITCOMB: No, not particularly. During my weeks on campus, I tended to come to work and spend my time in the LIGO areas on campus, but I didn't feel welcome on campus particularly. It was residual from all the Drever stuff.

ASPATURIAN: Too many unhappy associations with too many people.

WHITCOMB: Too many unhappy associations. So I would come to campus; I would park in a place where none of the other physicists parked, so that I wouldn't have to meet anyone from the division on my walk from the parking lot to my office. It was just I didn't feel good about being on campus.

ASPATURIAN: What years are we talking about? What is the time window here?

WHITCOMB: Oh, this is '97 to 2004, something in that range.

ASPATURIAN: Feelings were still that raw that long after?

WHITCOMB: My feelings were, certainly. Still are, to be honest. Even today, I don't really feel welcome on campus, except with some of the LIGO folks and some of the administrative people I have worked with. It may not be logical, but that is how I feel.

ASPATURIAN: Something you talked about at the Theoretical Sciences Center—I wrote it down, and I'm quoting here, I think—was the essential infusion of leadership from the high-energy physics community. You said that you felt basically that without that, this project would not have succeeded. You talked a bit about that earlier, in connection with Barry Barish, but I wonder if you'd like to elaborate on it, because I thought it was an interesting point.

WHITCOMB: If you look at what parts of the physics community have done big projects and where have they done those successfully—the ones where physicists have actually participated, as opposed to projects that are largely done by just engineering organizations—the one place where that has happened is really high-energy physics.

ASPATURIAN: I see, I see. The big accelerators.

WHITCOMB: Where the management is actually done by high-energy physicists. In the optics community, which is where a lot of the people who were engaged in LIGO in the early days came from, there just isn't much tradition of that large-scale sense. I think few of us really understood anything about how you manage a large budget, how you organize it, what's really required. Not only to succeed but also to convince your funding agency that you're succeeding, which is almost as important as real success, and not guaranteed. Real success doesn't guarantee that you convince your funding agency that you're making progress. Those two things are correlated but not 100 percent the same.

ASPATURIAN: Speaking of that, I think when we left off last time we had just about gotten to putting together the NSF proposal for Advanced LIGO. And also touching on the international collaborations that were getting under way, and you said you'd like talk about that this time. So would you like to start there and tell me what was happening and what you were doing? I think this brings us up to around 2002, or so, when the NSF proposal went in or was written.

WHITCOMB: I don't remember exactly when the NSF proposal for Advanced LIGO was written.

ASPATURIAN: It was approved in '04, so it must have been a year or two before that.

WHITCOMB: Yes. At least a couple of years before that. We had started installing the Initial LIGO detectors in 1998, so by '99, maybe 2000, we were well into the installation and commissioning of those detectors, and about that same time, Gary Sanders, in particular—the deputy LIGO director—saw the need to put together the proposal for the next-generation detector. And so he started to siphon off some of the scientists who had worked on Initial LIGO to work on the design and the proposal. And so it started off with Rai Weiss and David Shoemaker, from MIT, Ken Strain from Glasgow, and Eric Gustafson from Stanford.

ASPATURIAN: Ken was kind of a holdover from the Ron Drever group at Glasgow?

WHITCOMB: He was. He was a member of the GEO Collaboration but also a member of the LSC. Rai, Ken, Eric, and David Shoemaker were the leaders who in the '99, 2000 timeframe first put together a white paper on the design of a next-generation detector that would have better sensitivity. It was conceptually like Initial LIGO, just done much better, because of the experience we now had and the advances in technology. So Gary Sanders saw that as an opportunity, and he tried to foster and pull together a team of people to work on the NSF proposal for a next-generation detector. First the design of the detector, then a cost proposal, and then eventually a full-up technical proposal to the NSF.

David Shoemaker had been the deputy leader on the detector project while we were doing the design of the detectors, so he understood a lot about interferometers in general. But David didn't want to spend a lot of time at the observatories; and so, once the focus moved to the observatories for the installation, he saw this as an opportunity to contribute by working on Advanced LIGO rather than spending a lot of time at the observatories like I was doing. He refocused his attention to Advanced LIGO, and after Gary left LIGO, David became the Advanced LIGO project leader.

I felt like we really needed some level of concentration on the initial detectors to make them work, so I studiously avoided Advanced LIGO, not because I didn't think it was important, but because I didn't want to dilute my efforts on that. But a number of other people did shift and start to work on Advanced LIGO. It had the advantage that you didn't actually have to hop on an airplane and travel to the observatories, and so I think that's one of the attractions that led a lot of people to want to work on it. Also, it would be new and more sensitive and so on, and if you weren't really going to be involved in the Initial LIGO hardware, then that's the place you wanted to be involved.

ASPATURIAN: When you talked about hopping on an airplane, that raised a question for me. Did the advent of email and the ability to just do things and communicate more swiftly on the computer change what you were doing? You were working at these various locales all over the country, coming back to campus, going back to Washington, going out to Louisiana. Did this make a difference?

WHITCOMB: It certainly did. Of course, obviously you can't clean a mirror using email—

ASPATURIAN: No, you can't, but you can talk about what needs to be done that way.

WHITCOMB: But, you know, the electronic tools for doing conferencing, for sharing documents and so on, were improving at the time, and it made keeping people engaged easier. We installed an electronic logbook at the sites from the very beginning, so that instead of keeping paper logs where you have all of the notes about what you did, everything went into an electronic logbook that's visible to anybody within the LIGO lab. And so the first thing a lot of people on campus would do every morning is read the electronic logs to see what had happened yesterday at the observatory. That way they could keep in touch with things, and then when they do travel to the sites they're more useful and more engaged in what's going on. They don't have to spend two days catching up with what's been happening. So that was a huge step forward.

ASPATURIAN: I can imagine. It just strikes me this was such an enormously intricate project with so many moving parts, and unprecedented in its objectives. High-energy physics had years and years of precedent to draw on for their mega-collaborations, but you really had nothing. Or, not much.

WHITCOMB: Right. It was very different—for instance, this whole advent of an electronic logbook. Those things had been used in high-energy physics before, but most people in the optics community didn't do that. Of course, it's a little easier to just jot things in your own notebook than it is to open up a computer program and type in your notes, and be sure that you've got the data that you captured in a form that can go into this computer program, but once people recognized how it enabled a more collaborative venture, then they embraced it, I think, pretty well.

ASPATURIAN: Yes. And do you think it helped move the project along faster in the grand scheme of things?

WHITCOMB: Oh it definitely did.

ASPATURIAN: Were you involved at all in the process of submitting the Advanced LIGO proposal to NSF, or making the case? Was NSF expecting this?

WHITCOMB: Yes. My role in that was mostly to complete the Initial LIGO installation and commissioning and to get it to design sensitivity, which was a necessary precondition to Advanced LIGO funding. But NSF was expecting the Advanced LIGO proposal. The groundwork for Advanced LIGO had been laid in the '80s.

ASPATURIAN: I see. Robbie Vogt did this.

WHITCOMB: The ’89 proposal already envisioned LIGO as a multi-stage project that had an initial sensitivity for the detectors and a future sensitivity for detectors that was roughly ten times better and that would be installed in the same vacuum systems. Although there was not an *explicit* proposal for the Advanced LIGO detectors, the approval from NSF implicitly said, “We know we’re going to get a proposal for improved detectors. We don’t know the technology for it yet, but the proposal *will* come, and we have made a commitment to LIGO to take that second proposal very seriously.”

And so that had sort of been forgotten, and between ’89 and 2000, there was a lot of water under the bridge, but in about that 2000–2002, timeframe, people started reminding the NSF that this was a two-part program. There was a while when we even referred to this as LIGO 1 and LIGO 2, and then we were told by NSF not to call it LIGO 2, because that carries the notion that maybe there’s going to be a LIGO 3 and a LIGO 4, and that scares the NSF. So the thing that we used to refer to as LIGO 2 became Advanced LIGO. And when people walked up to the NSF and showed them the ’89 proposal, and said, “Look, it was there”—meaning plans for a more advanced LIGO—“and the review committees that approved the original project and the ones that followed have always been told, ‘This is just the first step in the process,’” NSF to its credit said, “OK, yeah; thanks for reminding us of that; you’re right; we did agree that we would move forward on this.” And so, when that proposal was submitted, it was taken seriously by NSF, and it was a good proposal.

ASPATURIAN: This was for an additional $200 million, I believe?

WHITCOMB: Roughly that, yes.

ASPATURIAN: How did this go over in the rest of the physics community? There’s only a finite amount of money NSF has to dole out, correct? So other physics projects, astronomy projects, perhaps, were not funded so that LIGO could be supported? Did you have any sense of this at this time?

WHITCOMB: I think that by this time the NSF had put in place an orderly process for these projects that we call MREFC—Major Research Equipment and Facilities

Construction. Those are projects that are outside of the normal research grant system. They're for building facilities that are at the 100 million dollar or larger level, or so.

ASPATURIAN: Really visionary projects, basically.

WHITCOMB: Yes. And what would happen is that the various divisions of the NSF propose those projects to the upper levels of the NSF, to a top-level committee there. Once they've reached a certain maturity and they've been reviewed and so on, then they get kicked up to this committee that consists of the NSF director, the deputy director, and the assistant directors, and they basically look at those; and if they say, "OK, yes, that project's ready to go; we're going to slot it into our list of projects that we're wanting to fund," then you basically wait for your turn to be funded.

ASPATURIAN: I see, I see.

WHITCOMB: So at that point, it's much less about competing with other physics projects than it is with astronomy or polar projects for the next polar icebreaker [research vessels that break up ice for field experiments at the poles]. So at that point, I didn't hear much resentment about the Advanced LIGO proposal from other physicists or astronomers. So that proposal, I guess, went in, was more or less reviewed and approved, and it got into line in the MREFC process, but with an asterisk beside it that said, it's not going to be funded until Initial LIGO reaches design sensitivity. Basically they said, "Show us that you can effectively reach the design sensitivity that was in that '89 proposal before we give you any money."

ASPATURIAN: Did that put extra pressure on you since you were the point person on the detectors?

WHITCOMB: Yes, it did put pressure on the LIGO Lab. Actually, by that time I had largely turned over the commissioning and detector improvement stuff to Peter Fritschel, who was running that activity. He's at MIT, and probably understands how the detectors operate better than any other single individual.

ASPATURIAN: Physicist at MIT?

WHITCOMB: Yes. Because by that time I had become deputy director, so we're not being very systematic in the timeline, but we're doing some different threads here. So I handed the commissioning stuff off to Peter, and he was under a lot of pressure. I was under a lot of pressure too, but not because I was in charge of that activity, but because I was the deputy director at that time.

ASPATURIAN: And you became the deputy director again in what year?

WHITCOMB: I was afraid you were going to ask that question. I think it was late 2003 or early 2004, something like that. [It was 2004. –*Ed.*]

ASPATURIAN: This was before Barry Barish stepped down?

WHITCOMB: It was. So, just to toss in one little additional detour: In 2002, I had been doing this commuting up to the observatory two-thirds time, and at some point my wife said, "This has got to end," and I had promised her that we would do something that was going to break the cycle of me spending so much time at the observatories. So I said, "We're going to go to Australia for six months," and we went to Australia in the last half of 2002.

ASPATURIAN: Was this in a professional capacity for you?

WHITCOMB: Yes, sort of. I had a visiting appointment at the Australian National University [ANU in Canberra, Australia]—

ASPATURIAN: Kind of like a sabbatical, in other words.

WHITCOMB: Basically, yes. Formally, research staff don't get sabbaticals, but I was determined to go. I have a good friend at ANU, David McClelland, who had given me a standing invitation for an extended visit. We shared some common research interests, so it seemed like a great way to recharge after all the stress of the installation effort. After I had agreed to Laurie that I would stop commuting to the LIGO sites, I started letting people know that I would be stepping back for a while and going to Australia. I hesitated to ask permission since I wasn't confident that I would get it, so I just started talking as if it was a foregone conclusion. I was willing to self-fund, using my accumulated vacation, and just taking a leave without pay for the remainder of the time. To his great credit, Barry didn't hesitate and told me to go without any question of whether my salary would continue. So LIGO continued to pay me, and I spent my time in Australia.

Stan and Laurie Whitcomb in Uluru, Australia, in 2002

I worked with the Australian groups, mostly at ANU, but also at the University of Adelaide and the University of Western Australia, on laboratory-scale things, and I spent time working with students and PhD students. They were also a part of the LIGO collaboration, so it was still working on that kind of thing. [*SW subsequently added:* David was the first, or at least one of the first, quantum optics experts to get involved in LIGO. He and his group were starting to do experiments about the quantum limits for gravitational-wave interferometers—concepts that had been around since the early 1980s, but that had never been seriously tried experimentally, like the light squeezing that we talked about last session. So it was natural for me to get involved with that work, and I was fortunate because not only was McClelland a real expert, he and his group were a real joy to work with.] I was working on squeezed light, so it was great fun; really a great break for me. I had been interested in the quantum limits to measurement during my first period at Caltech, in the early ’80’s, so I used this time to catch up on how things had developed since then, and to start to plan some experiments in that direction.

So I had a great time there. I came back from that literally at the end of the year—the 29th of December of 2002—having been away for six months. I’d handed all my other responsibilities off—mostly to Peter Fritschel—so I came back with kind of a clean slate. I decided I was going to do some quantum non-demolition work and also some data analysis. [*SW subsequently added:* Even before going to Australia, I had been talking about the quantum non-demolition ideas with Nergis Mavalvala during her time as a postdoc at Caltech. About the time I went to Australia, she got a faculty position at MIT and so when I came back to Caltech, my plan was to collaborate with her and her group on experiments there, rather than trying to start something new at for myself. I tried very hard to get Caltech to look at Nergis for a faculty position at the time she applied for the MIT position because I thought she was destined to be one of the stars in the field, but the response was that Caltech wasn’t ready to commit to any new faculty positions for LIGO at that time. But then, after she had been at MIT for a couple of years, Caltech decided to make her an offer to come here. I didn’t think it was a good move for her at that point in her career, and in the end, MIT gave her some perks that kept her there.]

I also got involved in a little bit of the data analysis for the burst group and worked with that for a year’s time.

ASPATURIAN: Burst group. What is that, exactly?

WHITCOMB: It’s the one that looks for the sort of signal you might get from a supernova core collapse and other things where you don’t know exactly what the waveform is going to look like. Supernovae were probably the source for which we had the greatest uncertainty in what to expect, so that appealed to my sense of wanting to find surprises. The group was pretty fragmented, and was viewed by some as a bit dysfunctional. After a few months, I found myself becoming one of the co-chairs of the group—I don’t quite remember how I got myself into that. But I am somewhat proud of the fact that we were one of the first analysis groups, maybe the first, to complete an analysis of data from the first science run of initial LIGO and get a paper published on it.

So I had settled into a fairly broad and rewarding role in LIGO. And then Gary Sanders announced he was leaving. He'd been offered the position with the TMT [Thirty Meter Telescope] project.

ASPATURIAN: Oh yes.

WHITCOMB: And so he, with rather short notice as far as I knew, announced he was leaving, and Barry, after a little bit of thought, offered me the position of deputy director, replacing Gary. And I think I took that very close to the end of 2003 or early 2004.

ASPATURIAN: How did you feel about that? Were you pleased?

WHITCOMB: I thought I was, yes. I thought, you know, suddenly here's a chance to try to make a difference. I really was looking forward to the challenges there, and it was great. We did some really interesting things during my tenure as deputy director. For example we got the Advanced LIGO proposal approved, and we created the Education and Outreach Center at Livingston, which is again, a great thing to have. One of the other things I am proud of during my tenure as deputy director was that we were able to bring Eric Gustafson to Caltech, more or less to fill the position that I was giving up. I think I mentioned Eric earlier, as one of the people who first proposed what Advanced LIGO would be. He was at Stanford at that time, and I wanted to get him to come to Caltech, but he was pretty senior and we never had quite the right job to offer him. So when I got promoted to deputy director, we finally had a position with the right stature. Eric played a crucial role in the design and implementation of Advanced LIGO.

ASPATURIAN: What were your responsibilities as deputy director? You mentioned the Education Center and polishing off the proposal. What were you doing with regard to the science?

WHITCOMB: Our mode of operation was a division of responsibility between Barry and myself, with Barry handling a lot of the public face of things, and the deputy director taking on kind of the internal management. I think the analogy that's sometimes used is the relationship between the president and the provost of the university. The president is the public face and sets broad policy agendas, and the provost is the person who does all of the internal work. And so it was kind of the same split of responsibilities. Barry managed all of the interface with Caltech and so on, and I worried about budgets and schedules and proposals and setting up reviews with the NSF, and ensuring that progress was made on the various activities.

And that was fine for a period of time, and then Barry said, "I've just been asked to be the leader of the global design effort for the international linear collider, and I'm going to go ahead and do that." He was also going to continue as LIGO director. He said, "I'm not going to have very much time to spend on LIGO because I'm busy managing this other big international thing, and so you just do stuff and when you really, *really* need me, come and ask me for something, but don't bother me unless it's really important."

ASPATURIAN: How did the LIGO organization feel about that? Their director suddenly, I don't know, probably taking 80 percent of his time and energy elsewhere? What did Caltech think?

WHITCOMB: I have no idea what Caltech thinks.

ASPATURIAN: You don't know what Caltech thought. OK.

WHITCOMB: I think Caltech's attitude was that as long as the LIGO project was going forward, they were fine with it. If that's what Barry needed and wanted to do, and we could manage it, that was fine. I think NSF was happy enough; I think they had a fair bit of confidence in me as a deputy director. I think it didn't bother the people within LIGO too much for the most part, again because I was a pretty familiar face, and I did try to be during that period of time very engaged with the LIGO lab and the LIGO sites. There was, I think, a little bit of a feeling broadly within the LIGO laboratory at the sites and at MIT that Barry and Gary had been at some level absentee landlords.

ASPATURIAN: Both of them?

WHITCOMB: Both of them. At that point in the project they didn't really spend a lot of time at the observatories. And I think there was some resentment at the observatories about being managed by leaders that didn't spend much time at the sites.

ASPATURIAN: I have a question about that, actually. Do you think that administrative style slowed progress at all?

WHITCOMB: No, not a lot; I don't think so in the sense that the kind of the detailed work that was going on didn't depend so much on them; Barry and Gary had set up chains of authority that provided good management for the day-to-day stuff. What it did do, though, was make people feel like big decisions were made and they weren't informed, that they weren't a part of them, didn't have any input, didn't learn about them until after they were made. I tried when I first took over as deputy director to have a policy that once every quarter I would go and spend the better part of a week at each observatory and at MIT.

ASPATURIAN: And so, this is interesting; they really didn't do much of this at all, even though these were essential collaborators? Didn't go to MIT much, didn't go to—

WHITCOMB: Barry did in the beginning, going every few months to the two sites and spending several days there to be available to talk to staff. But he got busy and stopped going as much. Gary did the same thing in the beginning, and then his visits started to be only for safety audits or NSF reviews, that sort of thing. But I made a point of just going, hanging out in an office there—modeled on what Barry had done in the beginning. I would work on whatever I needed to work on, but my door was always open. Anybody could wander by and say hi, stop in to chat about things, tell me what they're worried about; and I think that was a pretty positive sort of thing. And I tried to do that throughout the time I was deputy director.

ASPATURIAN: Did you discover anything useful using that approach that was of great benefit to the project?

WHITCOMB: Yes, I think I did. I can't remember what it was off the top of my head, but I think it made a difference.

ASPATURIAN: I was just wondering, because it does sound like a change in administrative style.

WHITCOMB: Then, unfortunately, as a part of that deputy directorship at one point—and this was about the time Barry was taking over as director of the ILC and stepping back a bit from LIGO—NSF had a budget cut. And they came to us and said "We're going to cut your budget by a million dollars, something of that order, this year and next. What are you going to do about it?" So we actually had to undergo a staff reduction.

ASPATURIAN: That must have been hard.

WHITCOMB: Which was hard. We had to let go some people who were on term appointments. We just let their terms end and did not renew them. We had postdocs that we wanted to keep longer, to whom we had to say "No, sorry, we're not going to able to; we'll keep you to the end of the term that you've been appointed to, but we can't reappoint you." And we had about four or five people that we had to actually lay off.

ASPATURIAN: Did this decision-making basically fall to you?

WHITCOMB: Yes. I made a recommendation to Barry, and he accepted it. I worked a lot, very closely, with Caltech HR [Human Resources] during that process. It was an extraordinarily difficult thing; I did *exactly* what Caltech HR said to do and ended up getting complaints for discrimination on age and gender by two of the people who were laid off, and I ended up spending almost all of my time and certainly all of my mental energy for close to a year defending against those complaints.

ASPATURIAN: They grieved? It was a grievance procedure?

WHITCOMB: Right. So I had complaints filed against me, and HR worked through the first version of that, and eventually the Caltech grievance procedure had Caltech hiring an outside attorney to sort of interact with all parties and give a recommendation. I spent enormous numbers of hours with this tough attorney from Beverly Hills, and I spent a lot of hours gathering documents, writing stuff, and so on. It was an enormously draining activity—I used to go home completely drained. It was one of the most tiring times in my career. I think it actually impacted the ability to keep the project moving forward. Barry wasn't around; he had approved these decisions that had been made but hadn't actually been deeply involved with them.

ASPATURIAN: So you became the point person for all of this.

WHITCOMB: Yes.

ASPATURIAN: That was an unforeseen complication.

WHITCOMB: Yes. In the end the attorney from Beverly Hills actually said, no; we had done everything right; there's no basis for the grievance; and then there was no legal suit in the end. But—to my own defense I didn't actually get proven to be wrong on that one. [Laughter]

ASPATURIAN: The grievances were thrown out, in other words. Stop me if I'm jumping ahead and missing something I should be addressing—but I think in 2005 you became LIGO's acting director? So Barry must have stepped down.

WHITCOMB: Yes. This grievance procedure ended about the beginning of 2005, and then in something like March or April of 2005, Barry suddenly basically said, “Wait a minute; I’m going to retire at the end of this year.”

ASPATURIAN: As a faculty member and as director of LIGO?

WHITCOMB: Well, he was going to step down as a faculty member, and then he couldn’t be director of LIGO anymore. It’s a little known fact—I’m not even sure I’m supposed to tell you this but I will anyway, because what can they do—they can’t fire me! [Laughter]

ASPATURIAN: Until you approve the thing, it’s all confidential.

WHITCOMB: So the deal is that of course there’s no mandatory retirement age for faculty, but they do want to encourage faculty to retire, and so one of the ways they do it at least in those days—I don’t know if it’s still true today— is that if you retire before you turn seventy, Caltech will dump two years of your salary into your TIAA-CREF account.

ASPATURIAN: I’ve heard something like that. They don’t do that for staff, let me tell you.

WHITCOMB: I know that. [Laughter] But they want the faculty to retire. So I think Barry may not have actually been aware of that, or maybe he was just made aware of it, because he went from thinking he was going to come back and continue with LIGO for a long time to suddenly, “No, wait a moment, I’m going to retire.” So I think he learned about this, quickly decided it was worth it, and so he announced he was going to retire at the end of the academic year in the spring of 2005. I was appointed as acting director because they didn’t have enough time to do a search and get a replacement, and so I was acting director for, I don’t know, a period of a few months.

ASPATURIAN: Could you have become director?

WHITCOMB: What happened was up to that point they hadn't decided what in the world to do. I think nobody had really thought about, "Well, you know, eventually we're going to need to replace Barry as director." They hadn't really thought about it. Formally, the LIGO director is appointed by the president of Caltech in consultation with the president of MIT. Of course, the presidents don't spend much time on LIGO, so they accept a recommendation from the LIGO Oversight Committee, which is a joint Caltech–MIT committee. But in those days at least, the committee—which was actually chaired by Tombrello—didn't really spend that much time on LIGO either. They didn't really have a good selection method, and they're busy people and didn't really want to spend a lot of time trying to find a director for LIGO.

So Tombrello who was PMA chair at that time, started casting around for a process that Caltech could go through, and I don't know exactly where the idea came from, but the ultimate idea was that they would ask the LSC—the LIGO Scientific Collaboration—to form a committee to come up with a recommendation for a LIGO director. Somehow they would interact with Caltech HR to get the applications for this, because it's a real job, and they would have to do a search that would obviously include Caltech and MIT faculty, but they would also have to search outside more broadly. I recommended Gary Sanders as the chair of that committee, because I figured there was probably nobody who actually understood as well as Gary what that job of LIGO director entailed. Plus, Gary had just taken had a job he liked, so he wouldn't likely want to be a candidate, which eliminates that as a potential source of conflict of interest. Barry had made it clear he wasn't going to be involved in that selection.

ASPATURIAN: He was on to something else by now.

WHITCOMB: He was on to something else; he didn't really want to get involved; and I think he had in principle some objection to it, which is that if you have the current director choosing his successor you run the risk of inbreeding. So by this time Gary had been away for several years, but he still understood what the job entailed, and I recommended him to chair that committee, and Tombrello adopted that recommendation.

Maybe he got it from other people as well; he might even think he dreamed it up himself, but I did make that recommendation.

Anyway, so they came up with a committee. It included Kip and Rai and other people—I don't exactly remember all of them. They put out a call and said, "Does anyone want to be the LIGO director? You know, you could apply." And then they started also looking themselves for people. You don't want to just wait around for people to apply; you actually want to be more proactive and think, Who do you really want to have do this? And actively recruit good people, not just wait for people to see the advert and come to you.

ASPATURIAN: Did you apply?

WHITCOMB: I did. They went out and got, I don't know how many applications—again, most of that's relatively confidential as an HR matter. At some point I was told that they were down to a shortlist of three people—myself; Karsten Danzmann, who was the head of the German wing of GEO; and Jay Marx, who had been on one or two of the LIGO review committees and made a good impression on people, and when people started asking around who would be a good person to take this job on, different people pointed them at Jay. So this LSC committee then focused in on the three of us; they did interviews and so on. At that time Jay was at Lawrence Berkeley Lab up in the Bay area.

ASPATURIAN: What was his field, specifically? Was he also in high-energy physics?

WHITCOMB: High-energy physics or accelerator physics. It's kind of an irony—I think most of the people who are real high-energy physicists would say, "No, he's not quite a high-energy physicist." He'd worked on something called the Time Projection Chamber with another guy who became really quite famous for it—what's his name, David Nygren. And then Jay ran something called the STAR detector at RHIC [Relativistic Heavy Ion Collider]—the design and construction of that—and then he led the construction of the Advanced Light Source at LBL.

ASPATURIAN: LBL? This is Lawrence Berkeley?

WHITCOMB: Yes. Lawrence Berkeley Lab. And, so he'd managed several large projects. He was somebody that I think Gary Sanders knew quite well. He was at that time one of the sort of people who best understood how to organize projects—how do you organize them if you want to screw them up, and how do you organize them if you want them to succeed. And he also had the reputation of being the sort of person who could go into a dysfunctional organization, see what's wrong, and figure out how to make it functional. Oftentimes it's by identifying that one key individual that's really gumming up the works and finding something else for that person to do.

So at one point during that whole process I had to be up in the Bay Area for a meeting with a company that was making some of the Advanced LIGO optics. The meeting was going to last all morning, and then I could either hop on an airplane and come home, or I could say, you know, "Maybe I will offer to call Jay and see if he just wants to have me come by and answer any questions he might have about this LIGO job that he's being interviewed for."

So I asked a couple of people on the selection committee if that would be acceptable and appropriate, and if they would object if I just called Jay up, and they said, sure, if you're willing to go up and do it. I didn't know him. He had been on a LIGO review committee, but I didn't remember him. Maybe I wasn't at that review; I'm not sure. So for me it was a cold call. I called him up and made this offer, and he was very gracious, really thanked me for it, and so I did actually drop by, and we spent the better part of an afternoon just chatting about LIGO—the different challenges; what are the good things and the bad things. I think he really appreciated me making the effort to do that. He knew that I was in competition with him for that job, but I think it was well worth it for everyone. So in the end the LSC committee did pick Jay, recommended him to the oversight committee, and the oversight committee rubber-stamped that, and it went to the [Caltech and MIT] presidents, who again rubber-stamped it.

ASPATURIAN: Do you feel the fact that you're not a faculty member was problematic for this group?

WHITCOMB: Well Jay was not a faculty member either. So I don't think that was a problem for that group. I have one very close friend on that committee who's told me that there were people on it who didn't think I really wanted the position and was just applying because—for whatever reason. So that may have been part of why I didn't get it. I think there was also a feeling that they already had me as deputy director. If we can bring in somebody new for director, gosh, then we've increased the talent pool, and—

ASPATURIAN: In other words, you were penalized for being too good at your job?

WHITCOMB: Not so much for being good at my job, but rather for being there already. In the end, I think they made the right decision, OK? I'm not sure I thought so at the time. I think I was penalized a little bit because I had been spending so much time on this HR matter.

ASPATURIAN: Something totally out of your control.

WHITCOMB: Something totally out of my control, but nonetheless it actually did impact on my ability to do the job that I was supposed to be doing. At this point I'm fine with it. Jay and I are actually quite close friends. He lives half a mile from where I do, and we get together every Friday morning and walk our dogs together. So we're actually good friends out of all of this. The one thing that I still seethe about was the way it was handled. Jay got his call on a Friday, offering him the job, and the next morning, on Saturday morning, I'm at home, and I get a call from him telling me that he'd been offered this job and wanting to know more about it, and wanting to ask more questions.

ASPATURIAN: Oh dear, that's not a good way to do things.

WHITCOMB: I kind of thought it was not a good way. I think Caltech owed me a clear statement that they'd decided to do something different. But they didn't even bother to tell me on Monday. It wasn't until Tuesday that anyone bothered to tell me that they'd made the offer to somebody else. That really, really, *really* hurt me, and still does.

ASPATURIAN: I can imagine. Were you at all tempted to leave the project as a result of this?

WHITCOMB: Well I— Because of the way it was handled—

ASPATURIAN: I would think it would be a natural reaction for you.

WHITCOMB: Well the one thing I did do was make the decision pretty firmly that I *wasn't* going to continue being the deputy director, and so I pretty much let Jay know that I'd had my fill of being the deputy director. I would certainly continue as acting director until he was engaged and really hired. I would continue as deputy director until he was in place and could hire his own deputy director, but I really wanted to be replaced. It was not because I didn't want to work with Jay; I liked Jay. When he called me, he thought I had known; he didn't realize I didn't know. He was excited about it, and he wanted to talk to somebody else about it, to share his excitement. Gosh, you know, that was fine, and I'm glad that he picked *me* that he wanted to share that news with.

ASPATURIAN: It was Caltech you were upset with.

WHITCOMB: Yes.

ASPATURIAN: Yes, I understand. And probably not for the first time in connection with this project and your multifaceted roles in it.

WHITCOMB: [Laughter] Yes, that's for sure. Anyway, I said that what I really want to do is— The deputy director job is by far the most—

ASPATURIAN: The most thankless?

WHITCOMB: The most thankless job. It's the gruntiest of the grunt work, and you get the least amount of credit and appreciation for it. I admire the people who've done that job on either side of me, and I feel sorry for them. We are really lucky that someone as talented and as conscientious as Albert [Lazzarini] was willing to take on the job after I stepped down. Albert had been one of the people Barry brought in to ramp up the project after he took over. Initially he worked as a system engineer for LIGO, which meant that he coordinated and documented the requirements for all the different pieces of the project. Sadly, that is a job that nobody notices if it is done well, and everyone notices when it is not. Albert did it well, so nobody noticed it, and he never gets the credit he deserves for his contribution.

I realized at that point that I had gone basically as far with my career as I was ever going to go. I'd risen as high as I was ever going to in the LIGO organization and in the scientific community. If I was never going to go any higher than the deputy director, it wasn't worth it to me to continue at that level, with all the frustration of that position.

ASPATURIAN: I understand.

WHITCOMB: So that's why I really backed away from it. I basically realized I wasn't going to go any further.

ASPATURIAN: So what *did y*ou become, in terms of your position?

WHITCOMB: I thought that I would just go back to being a regular old scientist. I told Jay, "Look, you're probably paying me 50 percent more as the deputy director than you would pay me as just an ordinary old scientist, so knock a third of my salary, and then I want to only work half-time, so cut that salary in half, and I will just be a half-time scientist kind of working along here and chugging away." And I basically went to half-time, at a significant cut in salary, but with the notion that I would only work on the things that I wanted to work on.

ASPATURIAN: This would have been around 2006?

WHITCOMB: Yes, it was about that time. It may have been a bit later—I don't remember the exact timing. I know I certainly told them to cut my salary, because I didn't want them feeling like they could drag me back into all of the personnel issues that I had been dealing with as deputy director.

ASPATURIAN: And I see here that you'd also achieved design sensitivity. So you had kind of reached your goal at that point.

WHITCOMB: Right. We reached it in 2005.

ASPATURIAN: So you'd done what you'd set out to do in some sense.

WHITCOMB: Exactly. So Jay said, "OK, you can be staff scientist here and continue to do whatever you want to do, and if you want to work with students or postdocs or whatever, that's just fine." Then somebody said, "Stan should have some kind of a title," so they dreamed up this title of chief scientist.

ASPATURIAN: It sounds good.

WHITCOMB: It sounds really good. It doesn't have very much meaning for most people, but it looks great on my business cards.

ASPATURIAN: A couple of things. You talked about the LIGO Scientific Collaboration. I'm not sure we ever established exactly what that was and when it was formed.

WHITCOMB: Right. It was formed, I think, in 1997.

ASPATURIAN: Right around the time that you gave Shirley your interview.

WHITCOMB: Right. So the idea was Barry's, as far as I know. He recognized that between Caltech and MIT, the groups that we had and the number of people at the two institutions who would be funded by NSF was not going to be sufficient to do the job. And moreover, to be honest, that the support within the broader scientific community wasn't going to be strong enough without engaging other universities.

ASPATURIAN: So this was his insight.

WHITCOMB: That was his insight. So he actually managed to get NSF to put together, again, a blue-ribbon panel to look at the exploitation and usage of LIGO, or something like that. It was chaired by Boyce McDaniel from Cornell. NSF put together this very high-level panel, which heard some presentations from LIGO and from Barry about the detectors and the potential and so on. And I think Barry laid out a vision where he said, "Look, I think we need a scientific collaboration." Again, he was going to a high-energy physics model, where Fermilab operates the accelerator, but a scientific collaboration operates the experiment that uses that accelerator.

ASPATURIAN: Or like CERN, for example.

WHITCOMB: Or CERN. And so he was trying to go to a model that had a scientific collaboration that was broad-based and incorporated a lot of people from other universities. If you're doing an experimental collaboration at Fermilab, there will be Fermilab scientists who are participating in that, but they have essentially the same role and status in that collaboration as somebody from the University of Illinois.

So that's what Barry, I think, was trying to strive for. And he got this committee to recommend that LIGO and NSF should create something like that. What that meant was NSF needed to assign some money to fund the individual groups, and LIGO needed to welcome other groups into this collaboration. Barry had the view that a laboratory is a very top-down–driven organization. Everybody's got a boss; everybody does what the boss tells them. And he's the boss of bosses, so all very hierarchical. But a scientific collaboration should have people doing the kind of thing that they think *is* most important. It's more self-driven; it's self-governing and so on.

So he had a set of workshops, and we had an organization called the LIGO Research Community. It sort of existed for a year and then kind of fell apart, but ultimately a lot of the same people who got engaged in this formed something called the LIGO Scientific Collaboration. Any scientists within the LIGO lab were automatically members of the LIGO Scientific Collaboration, but so were a lot of other people. So that's the organization that has grown up in partnership with the LIGO laboratory.

ASPATURIAN: I see.

WHITCOMB: Rai Weiss was the first spokesperson for that; he was appointed to that role by Barry, and his job was to get this thing started so that eventually it should be self-governing with elected spokespersons to run the collaboration. They should organize themselves with white papers and documentation and so on. It was a really great vision for this thing.

ASPATURIAN: How many institutions offhand?

WHITCOMB: In the LIGO scientific collaboration?

ASPATURIAN: Yes.

WHITCOMB: I think initially it was something of the order of fifteen institutions. It's currently up in the sixty to seventy range.

ASPATURIAN: Is every single person basically who was involved in that collaboration on the first couple of detection papers? The thousand authors that we hear about?

WHITCOMB: To first order, yes. The collaboration has some rules that say you have to work on LIGO 50 percent of your available research time to gain authorship on collaboration papers. You and the collaboration have to define what your available research time is around any other job. So if 50 percent or more of your research effort goes into the collaboration, and you're doing things that are useful for the collaboration as described in a set of white papers, then you're eligible for authorship. Also you have to do that for a year before you get added to the authorship list, and if you stop doing it, you continue on the list for one year. So there's a one-year lag, with the idea that since the papers come out from the work done the previous year, you ought to be getting your name mostly onto the papers that were created while you were active.

ASPATURIAN: By, say, 2005, when you kind of changed your job title and your role with the project, how many papers had you yourself authored or co-authored?

WHITCOMB: That's an interesting question. It was probably at that point on the order of fifty or sixty, something like that.

ASPATURIAN: So you generated a lot of research articles as well, while you were doing all this other work.

WHITCOMB: First of all, I actually had a life before I came to Caltech in infrared astronomy, and that probably brought in about fifteen publications from that period of time.

ASPATURIAN: OK, so that came too.

WHITCOMB: There was really nothing almost from the time I started in 1980 to 2000, or there was a very small number of papers. Then in 2002, we started doing the runs with Initial LIGO, and we did our first science run called S1. There were four search groups—stochastic, burst, inspiral, and continuous-wave. Each one of those wrote an upper-limit paper based on the S1 data. So there were four papers plus an instrumental paper that was written, and I was an author on all of them, even though I had absolutely nothing to do with the CW paper, and absolutely nothing to do with the inspiral paper. But I had contributed to the instruments so that gave me authorship rights on those papers, even though I had not been involved in the analysis or the writing of the papers.

ASPATURIAN: I understand.

WHITCOMB: And as co-chair of the burst group, I helped shepherd that paper through its writing and so on.

So in that S1 era, there was also an instrumental paper, which I was one of the lead authors on. But after that—in 2003, 2004, 2005—every time we would do a science run, each of those four groups would write one or two papers, most of which I had little to do with; and certainly after I became deputy director I had very minimal time to spend with the actual writing of the papers or the detailed analysis. All I did was keep the LIGO Lab machinery running.

ASPATURIAN: Sure.

WHITCOMB: So most of these people have lots of papers that they're on but really didn't have a lot to do with and a few papers that they really had something significant to do with.

ASPATURIAN: Once you went to this chief scientist position, how involved were you with the evolution of Advanced LIGO?

WHITCOMB: Not too deeply at all. At the time that Advanced LIGO was getting going, and David Shoemaker was starting to lead that, some of the other scientists were starting to get pulled away from Initial LIGO onto Advanced LIGO. But I felt very strongly that we needed to do a good job with Initial LIGO. And at times I felt a little bit like I was the only person who kept insisting we needed to do a good job with Initial LIGO, and so I used to joke that I was going to continue to work on Initial LIGO, and then I was only going to work on odd-numbered LIGOs.

When I went to Australia, I could have gotten involved in Advanced LIGO there, but by that time Advanced LIGO was already moving forward and I felt like I'd sort of missed the boat on that one. So I started thinking about the generation after Advanced LIGO, and started thinking about light–squeezing and other issues that I really had been excited about when I first got involved in this in 1980—all of the quantum non-demolition aspects of it.

ASPATURIAN: All the scientific aspects that appealed to you so much at that time.

WHITCOMB: Exactly. So I started really saying as a joke, "You know, I just don't work on even-numbered LIGOs."

ASPATURIAN: I see, yes.

WHITCOMB: And occasionally somebody will actually repeat to me that line about even-numbered LIGOs. So I didn't really have too much to do with Advanced LIGO. During the time I was deputy director, of course, it was one of the major activities getting going, and so I had in some sense an administrative role but not a deep scientific role.

ASPATURIAN: You were looking beyond Advanced LIGO to some extent.

WHITCOMB: Right. I was involved in some aspects of it, not a lot. At that time I was one of the people who knew a fair bit about optics, optical coatings, and so on, so I actually worked with the scientists and engineers who were supervising the fabrication of the optics and their coating, and I reviewed proposals from companies for doing our mirror polishing and so on.

ASPATURIAN: You weren't down in the scientific nitty-gritty the way you were with the first generation.

WHITCOMB: Right. I had some roles in doing reviews. Occasionally they would have a final design review of this or that system, and they would ask me to spend a week or so reviewing documents and trying to find any holes or weak spots in this design or that design. I would spend some time doing that kind of thing, but I wasn't deeply involved in Advanced LIGO.

ASPATURIAN: What was the feeling within the sort of the core collaboration, of which you obviously were a part, about whether or not this was really going to work?

WHITCOMB: Whether Advanced LIGO was going to work?

ASPATURIAN: Yes.

WHITCOMB: Oh I think everyone was pretty confident. Rai, I think, always was going around trying to think of all the ways that it could possibly go wrong. So he was probably one of the most skeptical, but I was pretty confident that we hadn't made any completely crazy design decisions, and that it was going to work. And it did. It really worked pretty well.

From left, Barry Barish, Stan Whitcomb, Gabriela Gonzalez, Dave Reitze, and Peter Saulson on the grounds of the National Academy of Sciences in Washington, D.C., not long after the first LIGO detection.

STANLEY WHITCOMB

SESSION 5

June 19, 2017

ASPATURIAN: Thinking about our earlier interviews, it occurred to me that you were in a lot of respects LIGO's institutional history over this entire period of time. How do you think that shaped your role? Did people come to you a lot for information relating to all kinds of things, historically and technically, more so that they did with others? Were you kind of a go-to person for all this?

WHITCOMB: Yes, you're right; in many ways I was the institutional memory for a lot of things. When Jay Marx took over as director, and I stepped down as deputy director, he and I developed a close friendship, but it also was in part based on the fact that I knew and had a lot of history, and he was pretty much brand new, and so he and I used to talk frequently about these things. He would ask questions about the history, and when there was some situation that he didn't quite have all of the background on, he would often come to me for that. Since the discovery, there have been a lot of people wanting to hear and talk about the history of LIGO— interviews with journalists and so on—and I frequently get tapped for those. I find a large part of my engagement now with people is trying to review and correct the histories that people are writing of LIGO. [Laughter]

ASPATURIAN: I see. I'll take that as a cautionary remark.

WHITCOMB: [Laughter] Just as an example, recently Peter Saulson wrote up a talk that he had given in India. Peter dates to the same era that I do, but he had to have that paper reviewed by somebody, and the publications and presentations committee frequently turns to me to review any talk or any paper that has to do with the history. So yes, I am spending a lot of my time recalling and remembering these things.

ASPATURIAN: With that in mind, what do you think would have happened had you decided to peel off from this project, say, a decade ago? Do you think your recusal or removal would have slowed things down?

WHITCOMB: Probably not. I mean—

ASPATURIAN: Maybe you're being a little modest, I don't know.

WHITCOMB: I mean, in general the progress that people make this month or next month depends primarily on their understanding of what went on in the last six months, and much less on where a particular idea came from five or ten years ago, or what was the original purpose behind this or that aspect of the design. People just know that it works or it doesn't work; and you do it this way, you don't do it that way, for reasons that are oftentimes buried in history.

From a technical point of view, going back and looking into where an idea originated or how people came to a conclusion is really not so important. It's interesting in many cases, but it's not so very important for current progress. And people are quite happy to make up reasons for things. For example, I frequently hear explanations about why the orientations of the two LIGO sites are the way they are, and they all sound very, very plausible. They just don't reflect the considerations that really went into things at the time.

ASPATURIAN: You said last time, and I wrote this down, that when work on Advanced LIGO started, "I felt like the only person who felt we needed to keep doing a good job on Initial LIGO." Why were you so invested in that aspect; was it the R&D?

WHITCOMB: There are a whole variety of reasons. Initial LIGO was something I started off with and that I had had the most impact on the design of, and I felt it was important—to me, at least—that it worked, and that it worked as well as it could and as well as it was designed to work. That was important to me. The second reason is I had a more optimistic view of its potential to really *see* gravitational waves than most other people did. Most other people had decided that Initial LIGO wasn't going to see anything, and we were just operating it because we had to, not because we really expected to see gravitational waves. I thought it was still our best near-term chance to see something, and if we did see something, I thought it would make our future that much easier.

ASPATURIAN: Good point.

WHITCOMB: The other thing was that Initial LIGO was done in a—what's the right word to use here? It was done in a style that didn't depend upon huge teams of people; it was done on a much more individual basis, with analytical designs, not driven so much by large computer programs that tell you how to design something. Now that's both good and bad. It's bad in the sense that oftentimes you miss things when you do that—if somebody makes a mistake in the design or forgets something, it doesn't work—but it also can be more efficient. The manpower that went into designing and building Initial LIGO was actually much less than the manpower that went into designing and building Advanced LIGO. The space program has gone through that kind of same thing. The early things were done with individual designers pulling things together, launching stuff. Sometimes it worked, sometimes it didn't. Eventually things got so expensive that everything *had* to work, and the amount of paperwork grew and grew and grew and grew, and the result is things get very expensive, but they always work.

ASPATURIAN: Do you feel you're more drawn temperamentally and intellectually to the more intimate, hands-on scientific and technical model you just described for early LIGO?

WHITCOMB: Yes, definitely—I mean, when I was a graduate student I had the option of going into high-energy physics with its large collaborations and its tradition of designing things and engineering them and always being sure that everything was going to work by doing lots and lots of paperwork and design verification and so on. I chose to go into a field that was much more about smaller teams working on things, which sometimes might work and sometimes might not work, but if you do them quickly and cheaply enough, it's OK to have a few failures. When things get too expensive or take too long, then you can't afford those failures. And so when I first joined LIGO, I thought it might be one of those kinds of things. Well— And so by the time Advanced LIGO came along, it was clear it was going have to be done in this much more rigorous, predictable, and professional way.

ASPATURIAN: On a high-energy physics model, in many respects.

WHITCOMB: And with the high-energy physics model, in many respects. I was not drawn to that particularly. Other people were, and that was just fine with me. I was concentrating on Initial LIGO, and that was OK with me.

ASPATURIAN: I wanted to ask you about some of the people you worked with in the later stages. You mentioned Alan Weinstein, who came aboard later, and I also wondered how you would assess what Jay Marx and David Reitze brought to the project as directors.

WHITCOMB: When Tombrello was division chair, one of the ideas that he had was that one way to integrate LIGO more into the campus and gain more support for it would be to engage more faculty.

ASPATURIAN: Right. You mentioned this, and that it didn't quite work the way he expected. [S*ee Session Three*]

WHITCOMB: And he recruited Ken Libbrecht and Tom Prince to do this. Ken took on a particular project, a postdoc, and some graduate students, and went off and worked on that project rather separately. Tom dabbled a bit with LIGO and then decided he wanted to do other things. Alan Weinstein came in a little later and in the beginning kind of took over the 40-meter interferometer. He served as the leader of that effort, guiding it and so on. Now Alan didn't have a lot of experience with interferometry—

ASPATURIAN: His background is high-energy physics, I think.

WHITCOMB: Right. And so it was a bit of stretch for him to take over the 40-meter interferometer. I'm not sure how effective it really was.

ASPATURIAN: There was a learning curve.

WHITCOMB: There was a lot of learning curve, and a lot of the stuff that went on there, I don't know how relevant it was. But, you know, I admire how Alan stuck with it. Eventually Rana Adhikari [professor of physics] was brought in as an assistant professor. Rana did have a lot more hands-on interferometry experience, so he took over the 40-meter, and Alan shifted more towards data analysis and large computing, which in fact I think may be closer to his experience in high-energy physics than the interferometry side of things. I think he gladly gave up the hardware work on the 40-meter to let Rana take that over, and that was a wise decision. I think Alan has done a pretty good job of mentoring a set of students and postdocs in the analysis of large data sets.

ASPATURIAN: What kind of large data is there to analyze in connection with this project?

WHITCOMB: The detector takes data around the clock, so there's a huge amount. There's only an occasional gravitational wave, but you have to look at all of the data, and you have to understand it in a deep way. It turns out it needs to be analyzed in many, many different ways, looking for binary black holes with many different combinations of masses, binary neutron stars with a different set of masses, and so on. So the computational challenges are really quite significant, and Alan dug into that and has, I think, done a pretty good job of managing that activity within the laboratory.

ASPATURIAN: So he found his niche in a very productive way.

WHITCOMB: I think he found his niche, which I think is better than the one which he originally took on, which was to manage the 40-meter interferometer. Let me shift and talk about Jay and then Dave. Jay came with no particular experience in gravitational waves, no particular experience in interferometry, but he did understand how large projects should be done and what the elements of a good organization are; and he had a reputation, and I think a well-deserved one, of being able to understand how projects get themselves into trouble.

So he stepped into the directorship role, and he did it in a very light way. He was not at all heavy-handed, no stomping in and rearranging things; he just stepped in and set to work. He recognized that Advanced LIGO was a major undertaking. He helped shepherd that project—David Shoemaker was the project leader; Dennis Coyne was the chief engineer; Peter Fritschel from MIT was the lead design scientist; and the three of them were managing the technical activities. Carol Wilkinson from Hanford was acting as the project manager in terms of managing the resources, budgets, and schedules—those kinds of activities—and Jay sort of oversaw that whole activity. And I think he did a good job. He managed those activities and kept the NSF satisfied. Jay did not really engage very deeply with Initial LIGO, and he didn't take a major leadership role in the LIGO Scientific Collaboration. Again, his personality is one where he's not a very out-front kind of person; he tends to lead by example rather than by direction.

ASPATURIAN: I can detour for one moment. So who did assume the primary leadership role in the scientific collaboration?

WHITCOMB: I think that was about the time that Dave Reitze took over from Peter Saulson as LSC spokesperson. Peter Saulson was probably still the spokesperson when Jay came, but shortly after that Dave Reitze took over. And he did step into this leadership role in the LSC, guiding it. I don't know that Jay ever felt totally at ease within LIGO, and that may explain part of why he tended to oftentimes come and talk to me about things. If you start to know Jay as a person, you find that he actually doesn't have the degree of self-confidence that he deserves to have. He tends to underestimate his abilities, and so I think that's part of why he didn't engage a bit more deeply with things. He continued as director for one term. I was a bit disappointed when he chose at the end of his one term to step down.

ASPATURIAN: Was that a faculty position?

WHITCOMB: He was a senior research associate.

ASPATURIAN: Which they now call a research professor, I believe; there's been some sort of nomenclature switch there.

WHITCOMB: Could be. I don't follow those things, but if you say so. So he was a senior research associate at the time. There's something that Jay doesn't get credit for, which he should. Which is that during his term, in addition to directing the lab and overseeing Advanced LIGO as they were building its detectors, Jay recognized that the future of the field was going to require relying on international networks.

Before he came, or maybe about the time he did, we had just consummated this agreement with Virgo to combine our data sets, analyze them jointly, and publish everything jointly. Effectively, although the two collaborations remained separate for building and operating their detectors, all of the results were done in a single LIGO–Virgo collaboration activity. [S*ee also Session One*] All the data analysis, all of the results. Much of that had been negotiated before Jay came, but Jay recognized that the future of the field required larger networks of detectors. Not just three detectors like LIGO and Virgo, but even bigger networks.

ASPATURIAN: Do you want to briefly explain why that is?

WHITCOMB: OK. An individual detector has no way of telling which direction a gravitational wave is coming from. The only way you get information is by looking at the arrival time in one detector and then a second detector. By knowing that the gravitational wave travels at the speed of light and knowing what the time difference is between the two detectors, you can determine the angle between the direction of the wave and the line connecting your two detectors. That means you can localize the source to a circle on the sky around the line connecting your two detectors.

To do better, you need three detectors; and when you do a more detailed analysis you find that a network of three detectors does a pretty good job of locating sources if they're perpendicular to the triangle formed by the three sites, but not so good if they're in the plane of the triangle. And when you think about a triangle, there's a lot more region in the plane of the triangle than there is perpendicular to the triangle. The way you get around that is by adding a fourth detector that you want to have far away from the other detectors. And there was beginning to be a broader recognition of that within the community.

ASPATURIAN: Was there also a financial element involved—the idea that it was better to have more institutions and maybe national institutions involved to support the scale of this science and technology?

WHITCOMB: Certainly. If you can get it, yes. But what we're really trying to do is not get more people sharing on a common expenditure, but more people on a larger expenditure, so you actually win in a big way.

ASPATURIAN: I see what you're saying.

WHITCOMB: So this concept of needing international networks was growing broadly in the community. There was an organization called GWIC, the Gravitational Wave International Committee, which brings together the leaders of all the gravitational wave projects around the world. When Jay became director, he automatically became a member of GWIC. I was the executive secretary of GWIC at the time.

ASPATURIAN: Once again, you were the institutional memory.

WHITCOMB: GWIC has a number of people with institutional memory. But GWIC decided that we needed to have a roadmap for the field. We were anticipating the next-generation detectors, looking toward the future, and we wanted a broad roadmap for gravitational waves, not only using the ground-based detectors but also the space-based detectors and pulsar timing—just a broad road map that would lay out what are the scientific goals, how do these pieces all fit together, why is each piece important.

So GWIC decided to commission this roadmap, and Jay was either chair or co-chair of the subcommittee that was writing it. And in that process he really became convinced that the future was international networks and that we really needed not just three detectors, like LIGO and Virgo, but a fourth and possibly a fifth detector. During this time, he and I were together at a conference in China, and after the conference banquet, we went on a boat ride along the river through the city, and Jay pulled me aside and said, wouldn't we be a whole lot better off if we could get Australia to build a vacuum system for the third Advanced LIGO detector that was being planned for Hanford so we could put it in Australia instead. And I said, of course we would be better off—we would never get the NSF to agree to it, but it would be wonderful. So Jay said, would you mind if the two of us talked to our Australian colleagues who were there on this boat ride with us; and that was the beginnings of what was proposed to be LIGO-Australia.

The idea was that if Australia would fund the site and the vacuum system and a place to house it and all the people to put it together, LIGO and the NSF would contribute the hardware for a LIGO detector that would be located not at Hanford, where the plan was to have two detectors, but in Australia. Much better for the network. So Jay proposed that; we talked a little bit about it, and over the next month or two, I sort of took over as the point person trying to make this happen from the LIGO side, assisting the Australians. I sort of oftentimes get credit for having dreamed up that idea, but it was really Jay's idea.

ASPATURIAN: Were you working with the same people in Australia that you'd worked with when you went on sabbatical there?

WHITCOMB: Yes. And that's why Jay grabbed me at this thing and said, "Let's go talk to the Australians." He didn't know the Australians well, but he knew that I did and would know who to talk to.

ASPATURIAN: Which institution were you based at over there?

WHITCOMB: I was based at the Australian National University, but worked also with the University of Adelaide and the University of Western Australia. And those three, plus a couple of others, were the primary ones that eventually proposed LIGO-Australia. At the time, I had agreed with the Australians that if they got funded for it, I would actually move to Australia and take over as the director of LIGO-Australia, so I was bouncing back and forth and spending time there trying to help them write a proposal and convince their government to do all of this. So I got very heavily identified with LIGO-Australia, even though originally it was Jay's idea and not mine. So there's at least one piece of history that I can correct here.

ASPATURIAN: So what became of LIGO-Australia?

WHITCOMB: Oh we wrote a dynamite proposal; we costed it; we planned. We spent a lot of time on this, and then the Australian government said, "Wait a minute, this is a lot of money, and you have never detected gravitational waves. And besides, we're busy with SKA—"

ASPATURIAN: What's SKA?

WHITCOMB: The Square Kilometer Array. A big radio telescope. "And so go away."

ASPATURIAN: They must be kicking themselves now. What timeframe are we talking about?

WHITCOMB: 2010, 2011. The Australians had at a previous time talked with people in India about participating in a joint project, and so they actually approached a group of Indians with the idea of inviting them to be international partners in this. The idea initially was that if India could put up 10 percent of the money and be a partner, that might help sell this to the Australian government. And we approached the Indians that the Australians had worked with in the '90s on a prior proposal and had a set of meetings, and we got the scientists there lined up to be supportive of being a junior partner in LIGO-Australia.

At that point in 2011, the Australian government hadn't said yes, but it hadn't really said no either, and so we took that as a good hard "maybe." Collaboration between Australia and India would actually have been good, in the sense that Australia really needs economic partners in the region and it would have been helping establish scientific ties between India and Australia. It would have been great, actually. The Indians had developed some enthusiasm for the project, and I had developed some close contacts there. Bala Iyer was particularly important, and he became a good friend. It's funny—he is the most unlikely character to play the leading role in bringing about LIGO-India, but he did. He isn't an experimenter; he's a theorist—professor at the Raman Research Institute in Bangalore—who did analytic solutions and approximations to the equations of general relativity. [*SW subsequently added:* He had never worked on a big project—probably never had worked with more than a handful of collaborators at a time. He was nearing the end of his career—they have a mandatory retirement age in India—so he would only be able to participate in it for a limited time. I used to joke that even in a country of a billion people, it would be hard to find a more unlikely leader for such an effort. But he had decided that it was his mission to bring a gravitational-wave project to India, and he was tireless in that pursuit. He used to pick my brains constantly about how to create and foster a project there, and he had such a polite and even-tempered manner that he was able to get people to do things to help the project in a way that was truly amazing. I really feel privileged to have had the chance to work with him.]

So, when the Australian government said, "No, we don't want to pursue LIGO-Australia," our Indian partners—the scientists, not the government—said, "Would you make us the same offer that you made to the Australians? If we could get the money from the Indian government to put up a site and a vacuum system and the people to do this, would you offer us the detector?" The initial reaction within the broader LIGO was "No. We know the Australians; we know they're capable of doing this. We don't know anybody in India; they haven't done many large science projects; how could we possibly do that?" And so we went through a process of really trying to evaluate the technical capabilities in India and the interest level in the government.

ASPATURIAN: The infrastructure, intellectually and technologically, I imagine.

WHITCOMB: Yes. And we spent the better part of a year evaluating whether or not we would be willing to try it, and eventually concluded that we would. We had presentations and discussions with the NSF.

ASPATURIAN: Was that a bit of an eye-opener for you, discovering the extent to which India, or in this case its scientific community, *was* prepared to function as a partner?

WHITCOMB: It was. I don't know what I expected from India. India's always going to surprise. No matter what you expect, it will surprise you—I think that's safe to say. There certainly is a lot of capability there. There were some places where there were huge deficiencies, and there are big differences in how the scientific community operates. It's completely different from the US system in many ways, and different from anyplace else in the world, but it's clear that they could do it if it was a national priority for them.

It took a long time for it to become that national priority. Even though the government kept sort of leading us along and saying, "Yes, we're going to do this," it never quite got approved, and then there would be a change in government, all that kind of thing. We spent a lot of time trying to make it work, and then finally it did.

ASPATURIAN: Is this with the discovery of the gravitational waves that they made a commitment?

WHITCOMB: You can judge—the official approval came just weeks after we announced our first discovery. That got it to the attention of the prime minister, who said, "Let's go ahead and do this."

ASPATURIAN: So as I understand it, you're kind of overseeing LIGO-India at the moment?

WHITCOMB: No, I'm not. I was while I was still employed. Once I retired, then it no longer made sense for me to be trying to act as that kind of an interface between LIGO and India.

ASPATURIAN: Have other countries or collaborations of countries now shown an interest also in putting up detectors with the discoveries that have been made?

WHITCOMB: Well, Australia has expressed some interest now.

ASPATURIAN: Would they be working with India on this, or—?

WHITCOMB: I think it's not quite clear what they want. It's not clear that all the scientists want the same things, and it's not clear that the scientists want the same things that the government might want. So there are a lot of discussions; I don't think there are a lot of conclusions yet. There's been some question about whether Russia might be interested. Chile might be interested; China might be interested. Argentina at one time was expressing some interest. A lot of these are individual scientists who are trying to get something going. And whether their governments would ever back them is not clear. To do something of this scale requires a real partnership between the scientific community and the government, and that's not easy to organize in a short period of time.

ASPATURIAN: So we're not likely to see LIGO franchises springing up everywhere anytime soon.

WHITCOMB: No, I don't think so.

ASPATURIAN: Interesting vision though. So what *are* you doing right now?

WHITCOMB: That's an interesting question.

ASPATURIAN: It's not my concluding question, by the way.

WHITCOMB: Right. So what am I doing these days? I'm trying to clean out my office and sort of fade away, OK? I'm trying to figure out what do people do when they no longer work all the time. I spend some fraction of my time trying to be sure that people who really contributed in major ways to this discovery get some sort of recognition for what they've done. I mean Kip and Rai and Barry and Ron are getting a lot of recognition, and deservedly, but there are a lot of others, many that we have talked about, who also deserve an attaboy; so whenever I see a chance to make that happen, I jump at it. Doesn't always work, but often enough to make it worth the effort. There's a huge amount of non-linearity in how much recognition people get for how much contribution they make. What do I mean by that? We have a thousand people in our collaboration; a thousand people on the authorship of this [gravitational-wave] discovery paper. I think if you asked people in the collaboration which scientists made the greatest contribution to the success of this whole effort, you'd get most people saying Rai and Kip Thorne and Barry and Ron. I'd actually put Robbie in ahead of Drever, though not many in today's collaboration are that familiar with his contributions. But there are a number of others—many of the people we have talked about in these sessions, and others, who made really crucial contributions. So, I spend some of my time trying to find ways to get some recognition for this group of people. I can't always find the right thing for all the people who deserve to be recognized, but sometimes I do.

And I get asked about the history of LIGO, like our sessions.

ASPATURIAN: I realize we talked about Jay, but we didn't talk about David Reitze and his role in all this.

WHITCOMB: Dave got involved in LIGO moderately early on. When Barry first really took over as LIGO director, one of his goals was to try to engage some other universities in helping build LIGO, and one of the first people he reached out to was a high-energy physicist he knew at the University of Florida, a guy named Guenakh Mitselmakher. Guenakh is something of a wheeler-dealer—

ASPATURIAN: With "macher" in the name, I would expect that to be true.

WHITCOMB: Well, yes, he's quite a— So he was quite enthusiastic, he really wanted to bring Florida in; he wanted to jump on this, and so on. The problem was he was a high-energy physicist, so he didn't bring any particularly relevant technical skills or any knowledge or anything like that. What Barry offered him was that if he could put together some kind of a group that would take on the subsystem of Initial LIGO, we could subcontract out to Florida to produce that hardware. This is a fairly typical high-energy physics model for how detectors are built. So Guenakh went off and recruited a few people. He recruited Sergey Klimenko, an expert in data analysis, to work with and prepare software for doing the data analysis, and also two optics guys from the physics department—David Tanner and Dave Reitze.

David Tanner was really a laser physicist, materials scientist kind of guy, and Dave Reitze did ultra-short pulse lasers. So they had some overlap with what we did; not an awful lot, but they were sincere, hard-working guys who said, "This looks like fun, we want to try to do this." They came here, and I think each one spent a sabbatical quarter- or half-year at Caltech, learning about LIGO. We agreed to let them take on a LIGO subsystem called the input-output optics, which in the end turned into the input optics only, and they did quite a credible job of it. Dave Reitze was a young guy at the time and worked really hard, and he was also trying to keep up his other laser work. Eventually he decided, "No, this is where I really want to spend all my time," and he got rid of his ultra-fast laser work and worked on LIGO full time.

When Peter Saulson wanted to step down as LSC spokesperson, there were only a half-dozen other names that would come to mind as being suitable for leading the collaboration. Dave Reitze was one of them, and he wanted to do it, so he stepped in and did two two-year terms as LSC spokesperson. It was during the transition between Peter and Dave as spokesperson when we signed the agreement for sharing data with the Virgo project, so Peter did much of the negotiations, but the actual implementation of how the collaboration would work had to be done by Dave Reitze. And that was a major coup, and I believe it was really quite an extraordinary thing that we did then, creating a collaboration out of what had been two competing groups. He got quite a bit of visibility, of course, within the collaboration, so when Jay announced that he was stepping down as director, Caltech went into the same kind of process as before, saying "Oh gee, how do we choose a new director for LIGO? Well, let's do the same thing as last time."

And so they again got the LSC to appoint a search committee; that committee sought applications and looked at lots of people, and came back strongly recommending Dave Reitze, and so he took over as LIGO director. Jay had to extend his term, which was originally five years, by about a year—maybe not quite a year—until Dave could actually move. Dave had a tenured faculty position at Florida.

ASPATURIAN: He gave it up to come here?

WHITCOMB: He went on a leave of absence. He may actually still be on leave of absence from Florida. He didn’t want to give up that tenured faculty position.

ASPATURIAN: I can understand that, sure.

WHITCOMB: And Caltech was willing to let him do that, but he moved here and his identification is almost completely with Caltech at this point.

ASPATURIAN: Did he bring any particular skill set or anything like that to the project, or was it pretty much running itself to a certain extent at that point?

WHITCOMB: The LIGO lab didn’t need much day-to-day management or changes; it’s a pretty good organization. Dave more than Jay was familiar with the hardware and the technologies, and, so I think, he maybe put more of his time into managing the detector commissioning and those kinds of things. He also inherited the LIGO-India initiative, which hadn’t quite gotten off the ground but was far enough along that it was a little too late to back out easily, and so Dave was fairly deeply engaged with all of that. And also with just getting Advanced LIGO running and planning out the observing runs.

ASPATURIAN: Over the last decade or so, where has Kip Thorne been in all this? Aside from being, to some extent in the public mind, the LIGO brand? What’s he been doing?

WHITCOMB: The truth of the matter is that since he formally retired from Caltech, which was probably about ten years ago—I don’t know exactly what the timing is, you can look that up probably.

ASPATURIAN: I can, yes [2009].

WHITCOMB: To be honest, since his retirement from the faculty, Kip has not been very active in LIGO. Even prior to that, he had turned most of his attention toward the questions of computing the waveforms that you would expect to see from a pair of merging black holes. That had been one of the major computational challenges in physics—to take the non-linear equations of general relativity, solve them numerically, and predict the waveforms of the waves. And Kip had devoted much of his time towards organizing, coordinating, and cheerleading that effort into success. He was working with colleagues here at Caltech and Cornell chiefly, but others as well, to help try to move that project along.

So in that sense he was working toward the interpretation of the LIGO data but was not deeply engaged with anything that was going on with the detector side of LIGO. If we ever said to him, "Kip, we need you to do this, or can you please come to this NSF review, or could you go give a talk at this place where we need to build support," he was always good to do those things if we asked him. But he had to be asked, and then you had to track him down and get him to read his email. He was busy. He got involved in his *Interstellar* project where he originally thought he was going to make a movie that included gravitational waves in a significant way. It turned into something completely different from what his original concept was, but he was having a lot of fun with it, and once you get involved with something like that you don't easily say, "I need to go and do something for LIGO." So yes, we haven't seen much of Kip.

ASPATURIAN: You know, looking back at LIGO's almost forty-year history—very turbulent, very intricate, very complicated—do you see specific, crucial points where things could have gone either way? There was potential for failure at numerous spots, and yet—

WHITCOMB: There certainly were times when it could have easily failed, and maybe it would have been resurrected, but in a rather different form. If Robbie had not stepped in when he did as director and brought together the Caltech and MIT teams, the project would have floundered. It wasn't going any place; it wasn't going to get done without somebody like Robbie stepping in. When Robbie was having his difficulties, if Barry had not stepped in— And, if you read Barry's oral history, it wasn't 100 percent clear that when Caltech asked Barry to do it that he was going to. He was really, I think, on the fence quite severely about whether or not it could be done.

During the time when Robbie was selling the project to NSF, and the NSF had put together their own idea of a budget and Robbie insisted that it was going to be more expensive, that could have been a time when there was an impasse that would have kept the project from moving forward. Eventually the NSF dreamed up some creative ways to give the amount of money that was needed [*see Session Two*], and so the NSF showed a lot of initiative and took some big chances that made this thing happen.

So there have been lots of different places where the whole thing could have gone completely off the rails. Probably since Barry took it over, it was enough of a machine, and there was enough momentum, that it would have been almost impossible to derail.

ASPATURIAN: But the turmoil it went through in its early years would have been enough to knock most projects permanently off the rails, and yet that did not happen with this one.

WHITCOMB: I think it's because there was a challenge, the cachet of Einstein, and of seeing something that, you know, had been predicted for so long without being seen. I think that one way or another it was going to happen.

ASPATURIAN: I think, for example, of the Superconducting Supercollider, which ended up as a hole in the ground. But that did not happen in this case.

WHITCOMB: It did not happen, but the target that SSC was after, which was the Higgs boson, was picked up in Europe and moved forward. If the US had not moved forward on LIGO, would the rest of the world have picked it up? Probably. I think there's just too much challenge, a much too attractive challenge, to be ignored.

ASPATURIAN: How do you account for your resiliency throughout this whole saga? I mean it's quite—

WHITCOMB: Well probably I burned my bridges early on, and so I couldn't really do anything else after this. [Laughter]

ASPATURIAN: That's an interesting answer. Not one I expected. Do you see a moral to the whole LIGO story? Is there a take-away?

WHITCOMB: No. [Laughter] I don't. I can tell that we're really getting to the really difficult questions now. Yes, I think there is one in one sense, which is that when there is something that really is an intellectual challenge, something that is unknown, unproven, unexplored, there is a drive for people to explore it, to understand it, to take it apart and really figure out what makes something work. I mean, my guess is, if you were home, that there are very few unopened boxes in your home—

ASPATURIAN: That's true.

WHITCOMB: —because when you see something like that you, want to know what's inside. I can put a box in your home, and I guarantee within a few days, you'd figure out a way to open it up and look inside of it. People are just curious. They want to understand this. And here was this idea from this great mind, Einstein, who said, "I think that space is very different from what our experience is, and this is a way of testing that, of trying to understand it, of trying to figure out: Do we really know what's in that box?" And people are going to figure out a way to do it. I think it's just inconceivable that there could be something of that kind of revolutionary character that just never gets tested because people say, "Aw, it's too hard," or "it's too expensive," or "I don't want to spend thirty years of my life doing that kind of thing." It's just inconceivable that there wouldn't be people that would make it happen eventually.

ASPATURIAN: I have one more question for you, but first I want to ask if there's anything else you'd like to put into the record?

WHITCOMB: Not until after I hear your last question. [Laughter]

ASPATURIAN: How would you like history to assess your role within LIGO?

WHITCOMB: That's a really good one— I hadn't expected you to ask that question.

ASPATURIAN: Ah. Good.

WHITCOMB: And I could answer it in several different ways—

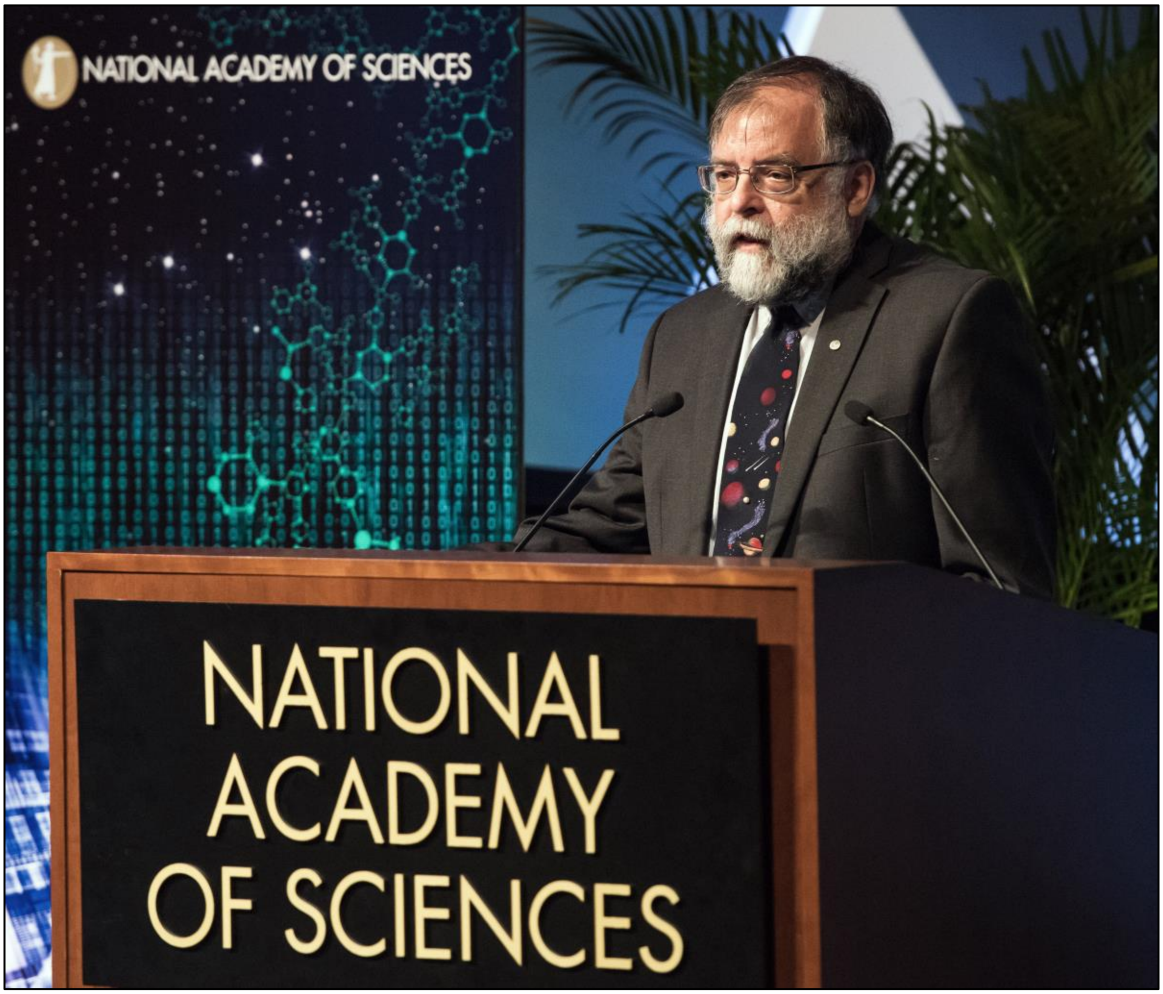
ASPATURIAN: Please do.

WHITCOMB: And they will all be right and contradictory, at the same time. At some level, on one hand, I would like to sort of just slip out without being particularly evaluated or having history remember or evaluate or assess my role. I mean, I've tried to make a contribution; I have my strengths, and I have my limitations. There are people whose good opinion I value, and I sort of think that they have a higher opinion of my contributions than I really deserve. There's a part of me that just wants to get out of this without them recognizing how limited I am and losing their good opinion.

Mostly, though, I think I was the right person—maybe not quite *the right* person—but *the right enoug*h person at the right time in the right place to do something. It's actually been really great. I think what LIGO has done is remarkable, and I'm really, really pleased to have been able to make a contribution to it. I think my contribution has been on balance positive, but it probably comes mostly from being in the right place at the right time, not particularly from being a great scientist or a great intellect or a great mind.

ASPATURIAN: Anything else you'd like to say?

WHITCOMB: I think that's it, thank you.



Stan Whitcomb speaks at the National Academy of Sciences after being awarded the Academy's 2017 Henry Draper Medal, which he and Barry Barish shared for their leadership of LIGO.